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VARIETIES AND LEVELS OF CLEARNESS CORRELATED WITH EYE-MOVEMENTS

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The present study arose from an attempt to develop an objective method of measuring the attention value of visual stimuli using photographic records of eye-movements as the criterion of attention. The validity of such a method will be discussed in another report. During the research on eye-movements, we were naturally compelled to use older acceptable criteria of attention, such as reports of attributive clearness and efficiency of cognition. We also depended upon some well accepted conditions of attention, such as variations in size, position, isolation, and novelty, to control the attention of the subject while eye-movements were being recorded. From these experiments we learned much concerning the relationship between attributive clearness, cognition, and eye-movements, and how these factors are influenced by the conditions of attention. We also obtained evidence bearing upon the old and troublesome question of levels of clearness.

Brief historical comments are essential to give significance to the account which is to follow. Titchener first proposed that attention (regarded from introspective observation) is basically a matter of sensory clearness.[1] Clearness was proposed as one of the fundamental attributes of experience. Attention is a term standing for the fact that at any moment an individual's consciousness possesses various degrees of prominence, or vividness, or clearness. Typically, the field of consciousness is divided into two main levels of clearness, either one of which may rise or fall from moment to moment. He admitted that the upper level may as a rule be uneven in clearness, having 'wrinkles' or 'wavelets' of clearness within it, but he remained in doubt as to whether there are also waves or wrinkles in the lower, obscure, level.[2]

* Accepted for publication December 5, 1934.
[1] E. B. Titchener, *Elementary Psychology of Feeling and Attention*, 1908, 162.
[2] Titchener, *A Textbook of Psychology*, 1910, 290.

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In an experimental attempt to test Titchener's doctrine of attention, Britz came forth with the conclusion that attention is equivalent to degrees of consciousness or awareness.[3] He also obtained reports of as many as twelve levels or degrees of awareness. Titchener in reply,[4] reminded Britz that he (Titchener) had made a distinction between attributive clearness, which is the basis of attention, and cognition, which as he had previously stated "is not clearness, it is an associative process of the assimilative kind."[5] Britz' instructions to his observers, he maintained, predisposed them to reports of degrees of cognition. Dallenbach supported the latter contention in a repetition of Britz' experiments.[6] Dallenbach gave instructions requiring a double task of the observers, one task being to "name and describe what you have seen" and the other to "give as exhaustive a description as possible of the processes involved . . . particular emphasis being placed upon the report of attributive clearness."[7] The internal conditions of the observer were thus favorable for arriving at two types of clearness. About 81% of the reports included mention attributive clearness. Of all reports, 19% made no mention of the number of levels, nearly 52% were of one level, over 28% were of two levels, and less than 1% of three levels. Dallenbach agrees with Titchener that multi-level reports are probably due to a confusion of levels of attributive clearness with levels of cognitive clearness. Of the latter he found as many as 17 to match Britz' 12. He also points out that in vision we may have two other varieties of clearness which may be wrongly identified by an observer as attributive clearness.[8] One of these is the clearness of the foveal region as contrasted with the obscurity of the peripheral region. This he calls 'physiological' clearness; an unfortunate term, since it is so general. The term 'foveal' clearness would be a better name for it, and this term will be used by us in the discussion to follow. We agree with Dallenbach that it is a very important factor to be dealt with, and it is a possible disturbing element unless eye-movements have been eliminated and unless the visual material is so arranged as to fall within retinal areas of equal discriminative power. The other variety of clearness which may be a disturbing factor Dallenbach calls 'physical' clearness. This is due to spherical aberration of the lens of the eye. Since this is a constant factor for each adjustment of the pupil, and since it can scarcely give one part of the visual field clearer definition than other parts, it is of small importance in dealing with problems of clearness.

Another source of confusion has been the attempt on the part of some to identify attention with the figure-ground phenomenon. Titchener had explained the shifting of an ambiguous figure, such as the brain-babies picture, as a shift in clearness; the forms that are perceived at the moment occupy the upper level of clearness and the non-perceived forms the lower level.[9] Several writers have denied that the figure is either universally or necessarily attributively clearer than the ground, or that attention must be given to the figure as opposed to the ground. Among these writers are

[3] C. A. Britz, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit*, 1913.

[4] Titchener, The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 43-61.

[5] Titchener, *Feeling and Attention*, 239.

[6] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230.

[7] *Ibid.*, 187.

[8] Dallenbach, Dr. Wever on attention and clearness, this JOURNAL, 40, 1928, 338 f.

[9] Titchener, *Textbook*, 277 f.

Rubin,[10] Koffka,[11] and Helson.[12] Wever, after an experimental attack on this subject, concludes that there is but one kind of clearness.[13] This conclusion is based on the very high correlation between figure-ground and attributive clearness but in the face of the fact that for one *O* (among three) figure and ground were reported equally clear in about 32% of the cases and the ground actually clearer than the figure in 3%.[14] Wever's definition of the one kind of visual clearness follows: "The focally clear portion of a visual experience is . . . that part of the field which stands forth in experience, which obtrudes itself upon consciousness, which is distinct and prominent about the undifferentiated background, which can be pointed to, grasped, described, reported definitely upon, which persists in memory."[15] The marginally clear portion has the opposite properties. From this general description and from the protocols from which it was drawn it would seem to the writers that at least three varieties of clearness are plainly indicated. Besides the attributive clearness and cognitive clearness already postulated by Titchener and Dallenbach, a third exists. This third type may be called 'figural' clearness; it is essentially a clearness of form or sensory organization. Some forms stand out sharply and distinctly from their backgrounds, well delineated and complete, whereas others lack this property. This seems equivalent to the 'precision' or *'Pragnanz'* of Gestalt theory. It might be regarded by some as a species of cognitive clearness; but it would seem to us more appropriate to reserve the term cognitive clearness to apply to the clearness of meaning or to reportability. Reportability of an experience may, and indeed often does, depend directly upon the precision of form, but the correlation is probably not by any means perfect, and therefore the two are distinguishable. Clearness of form or figure may depend to some extent upon what Dallenbach calls 'physical' clearness and certainly it depends highly upon his 'physiological' (our 'foveal' clearness), but no one would maintain that this makes those forms of clearness identical. 'Figural' clearness is conditioned also by cortical factors, and by cortical factors other than those controlling attributive clearness and those controlling clearness of meaning, *i.e.* cognitive clearness.

Chapman has recently suggested that we again ignore these various distinctions and rally under the banner of one undifferentiated form of clearness which shall be defined as cognitive clearness is usually defined, *i.e. "determinateness or adequacy of experience in respect to agreed-upon properties, and measureable by the reportability* (for example) *of such properties"*[16] (italics his.). As it turns out, however, the "properties" of the above definition would include any varieties of clearness which we could differentiate, if "agreed-upon." We are ready to grant that the observation of attensity or of any other property of experience requires cognition. Introspection is a cognizing of mental processes and their properties. Chapman's proposal therefore leaves us where we were before, except that it perhaps helps to 'clear up' the meaning of introspection.

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[10] E. Rubin, *Visuelle Wahrgenommen Figuren*, 1921, 96-101.
[11] K. Koffka, Perception: an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 553-570.
[12] H. Helson, Studies in the theory of perception, *Psychol. Rev.*, 39, 1932, 44-72.
[13] E. G. Wever, Attention and clearness in the perception of figure and ground, this JOURNAL, 40, 1928, 51-74.
[14] *Ibid.*, 61.
[15] *Ibid.*, 67 f.
[16] D. W. Chapman, Attensity, clearness and attention, this JOURNAL, 45, 1933, 159.

Apparatus and Technique

The stimulus material was prepared in the form of slides for an ordinary Bausch and Lomb projection lantern. Letters were typed upon *Radio-Mats* and geometric forms were glued to the same transparent sheets which were bound between cover glasses. There were 4 different types of slides, 6 of each kind, involving the 4 conditions or factors of attention: *position, isolation, size,* and *novelty.* In the 'position' slides, 9 capital letters were arranged evenly in three rows and three columns. In the 'isolation' slides, a single letter was placed on the right or left middle position and from 7 to 12 letters were bunched in a corresponding place at the opposite end of the slide. In half the slides the isolated letter was at the right and in half at the left. In the 'size' slides, 5 regular capital letters were typed on the right or left part and a group of 5 extra large capitals on the other side. In both cases the 5 letters were arranged in domino fashion, with the large letters forming a square 1.7 times the size of that for the small ones, since that was the ratio of the sizes of the large and small letters. The large letters appeared at the right and left an equal number of times. The 'novelty' slides contained 5 letters on one side and 5 unusual objects on the other. Among these unusual objects were geometric forms in silhouette each equal in size to the letters, colored squares or circles, and numerals. In every case the material was balanced as symmetrically as possible about a central point on the slide where the preëxposure fixation was to be held.

The material was projected from behind a Trans-Lux screen. The horizontal width of the exposure-field was approximately 9° of visual angle on either side of the line of sight. The light of the projection lantern was reduced by using a 100-watt bulb in place of the customary 500-watt bulb. It was further reduced by using a bluish cellophane filter. The exposure device consisted of a Betax No. 5 camera shutter which had been fitted over the lens barrel of the projection lantern. The shutter was set to give exposures of 100 σ, a time which is ordinarily too short for an eye-movement reaction to the stimulus.

The pre- and post-exposure fields were illuminated from behind by a 25-watt frosted bulb. This was necessary to avoid after-images from the exposure of the slides. A preëxposure fixation-point was provided on the screen since we wanted every exposure to begin with the same adjustment as to foveal clearness. An eye-movement camera of the Dodge type photographed the corneal light from the left eye. Horizontal movements were photographed. The horizontal division of material on the slide was made for this reason. Observations were made with both eyes and it is assumed that because conjugate movements occurred the record of one eye is a reliable indicator of the behavior of both.

It was decided not to give *O* the double task of reporting upon attributive clearness and upon meaningful content at the same exposure. During one series of exposures, therefore, the one kind of report was emphasized and during a second set of observations the other kind of report was stressed. The set of 24 slides was exposed to each of the 5 *O*s twice under two instructions. Series I and IV were under the attributive instruction and Series II and III under the cognitive. The order of the slides was alternated, with complete reversals and with a new starting point each time. Since photography of eye-movements requires extra time and since very complete reports were recorded after each exposure, only about 10 exposures could be made during each experimental hour.

The 'attributive' instructions were as follows:

Fixate the spot on the screen until the exposure. At the signal, 'Ready, now,' you are to be ready to observe the exposure-field and to report which part of the field is most prominent or most compelling, most intriguing, or the part you feel inclined to look at.

The 'cognitive' instructions were:

Fixate the spot on the screen until the exposure. At the signal, 'Ready, now,' you are to be ready to observe the exposure-field and report what you have seen. Name or describe as many objects as you can observe.

It will be noted that we did not use the terms 'attributive clearness' or 'cognitive clearness' in either instruction. Two of the 5 *O*s knew the meaning of those terms and one was at the time taking a course on attention. The three others were entirely new to the meaning of the term attributive clearness although they had had at least an introductory course in psychology. Only the two had previously observed in any psychological experiment and none had observed before in a problem on attention.

After an observation under the attributive instructions, *O* was handed a card 3 in. × 5 in., and was told to draw thereon the various areas of 'clearness,' this term being avoided almost entirely, and to label those areas 1, 2, and 3, to indicate the patches of greatest, next greatest, etc., vividness. He was encouraged to write down anything else he saw and to make a full verbal report concerning his drawing, the order of events during the exposure, and other points which he considered important. These reports were taken down in shorthand and later typed in connection with the sketches. All *O*s very readily reported patches of varying degrees of prominence or clearness and were usually positive as to the extent and shape of those patches.

Following the exposures made under cognitive instructions, although a report of objects was called for and was recorded first, *O* was also given a card upon which to sketch any patches of heightened clearness that he may have noticed. *O* very readily fell into this routine also. In neither case was mention made of clearness as such or of eye-movements, but some reports of both were voluntarily made and recorded by us.

The typical exposure was made as follows. *O* was seated in an armchair in front of the camera and grasped a biting-board with his teeth. The room was darkened, except for the lighted area of the preëxposure field, and the beam of light trained upon *O*'s left eye. One *E* (E_1) focused the camera and started the motor which moved the sensitized photographic paper.[17] During this time *O* held his fixation as best he could upon the spot on the screen. After about 6 sec., the other *E* (E_2) would say "Ready" and after 1.5 to 2 sec. "Now" and then trip the camera shutter. At the signals, "Ready" and "Now" and also at the opening of the shutter, E_2 would press a key which flashed a signal light inside the camea leaving its impression on the film along side of the eye-record. The camera was left running for 4 or 5 sec. following the exposure and then stopped in order to permit *O* to make his report. We thus have a record of what the eyes were doing before the exposure, at the preparatory signals, at the exposure itself, and following the exposure. The speed of the film was fixed at 1 cm./sec. by means of a constant speed motor. No very delicate time measurements are possible in this type of eye-movement record, since

[17] We are very much indebted to Mr. Ray C. Hackman who kindly assisted throughout most of the experiments.

an up or down movement of the eye may displace the eye-spot with respect to the signal-spot on the film, and hence give a false impression of just when a movement occurs. E_1 was able, however, to watch through a peep-hole in the camera box the positions of the eye-spot and the signal-spot at all times and to make a note of those few instances in which the eye-spot either moved to an advance position or fell to the rear of the signal-spot. The reading of the record was later made with these discrepancies in mind.

Results

Relation of the eye-movements to type of instruction. To our knowledge, this is the first attempt that has ever been made to find what the eyes are actually doing before, during, and after a tachistoscopic exposure. The best previous work has seen to it that the material so presented falls within the visual angle within which discrimination is relatively good and uniformly distributed. *O* has been given a fixation-point with the assumption that he can hold his eyes in one favorable position for receiving the impression. It has further been assumed that variations in instruction make no difference in *O*'s ability to hold his fixation. Although exposure-times are usually so short as to preclude an eye-movement during the exposure itself, this has not always been true. Our data show that these assumptions are not fully justified. The instability of fixation during tachistoscopic observation actually introduces important 'chance' factors into the determination of the results.

TABLE I

The Number of Saccadic Eye-Movements Under Attributive and Cognitive Instructions

Set	Before Exposure	After the Exposure			Total After	Total Before and After
		1st sec.	2nd sec.	3rd sec.		
Att.	376	86	61	55	202	578
Cog.	428	193	118	97	408	836

We counted the number of eye-movements during the preparatory interval, which extended from the "Ready" signal to the exposure itself and covered a period of from 3 to 4 sec. We divided the post-exposure period into three periods of 1 sec. each. Vertical as well as horizontal eye-movements could also be detected by breaks and overlappings in the eye-line on the sensitized paper. The results represented in Table I come from 5 *O*s each one of whom observed 48 tachistoscopic exposures under each of the two instructions.

Several facts are very clear from Table I. There is much shifting of fixation before the exposure. From the pooled results (each of the above values is based upon 240 exposures), it would seem that the average

individual makes from 1.5 to 2.00 saccadic movements during the preparatory interval of about 3 sec. There are marked individual differences in this respect. One *O* made totals of 3 and 8 movements under the two types of instruction respectively, whereas another made 159 and 191 during the same number of exposures (48). There are approximately as many movements preceding the exposure as following the exposure, if not more. Either before or after the exposure the cognitive instructions invariably induce a greater number of movements. Before the exposure the cognitive set induces about 14% more movements and after the exposure about 100% more movements. The only single exceptions to these last generalizations are in the case of one *O* who had equal numbers of movements under the two instructions during the second and third seconds after the exposure. It will be noted that in general the differences between the two instructions grow less as time lapses following the exposure. Before these rules are generally applied, let it be said that the exposure-material used here spread beyond the region of clearest vision, although the *O*s maintained that they could read all the letters without eye-movement if time were permitted. Nevertheless different results might be obtained if the material were more closely grouped in the foveal region. The eye-movements made under either set are undoubtedly in part searching movements as one aspect of the preparation to observe visually, and the instruction to name objects requires a more detailed scrutiny of the whole field.

Preëxposure eye-movements and reports of clearness. In view of the large numbers of preëxposure eye-movements, and since there are more under the cognitive set than under the attributive set, it is of interest to find the amount of correlation between the direction of the significant eye-movement, right or left, and the side of the field yielding the higher level of attributive and cognitive clearness. If a high correlation does exist, it might show that the post-exposure report is influenced by a preëxposure set to observe either on the right or left side of the field. The data were accordingly arranged in a nine-fold table both for the attributive instructions and for the cognitive instructions. Along one dimension of the tables were the categories: Eye-movement Left, No Eye-movement, Eye-movement Right. Along the other dimension were Greater Attensity Right, Equal Attensity, Greater Attensity Left, for the attributive instructions; and Greater Cognition Right, Equal Cognition, Greater Cognition Left, for the cognitive instructions. Coefficients of contingency were then computed after the method described by Garrett[18] and these were transformed into

[18] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 195 ff.

equivalent Pearson r's by correcting for class means by a method described by Kelley,[19] assuming rectangular distributions.

The correlations between the direction of significant eye-movement before the exposure and the side of the field having the greater attributive clearness during the exposure was only 0.045. The similar correlation between eye-movement and cognitive clearness was only 0.075. Both coefficients are based upon 240 exposures. Thus, if the direction of a significant eye-movement before the exposure indicates at all a set for either side of the field, we would have to conclude that such a set is entirely ineffective in controlling either attributive or cognitive clearness. Having already the conviction from many previous investigations that mental set *is* a very

TABLE II

AVERAGE LATERAL DEVIATION OF THE EYE FROM THE FIXATION IN TERMS OF MILLIMETERS AND AVERAGE LATENT TIMES FOR THE VARIOUS *Os*

	Attributive instruction				Cognitive instruction				Critical ratio
O	No.	Mean extent	P.E.$_{av.}$	Mean time	No.	Mean extent	P.E.$_{av.}$	Mean time	(in terms of P.E.)
H	18	.76	.099	.45	22	1.37	.216	.26	2.121
N	30	1.88	.125	.64	45	2.59	.119	.22	4.099
A	13	.56	.113	1.00	37	.71	.048	.54	1.271
S	29	1.53	.067	1.04	43	1.56	.094	.66	0.263
W	12	1.14	.185	1.06	16	1.96	.039	1.04	4.220
All	102	1.32	.474	.83	163	1.67	.652	.49	2.395

strong controlling factor for either attributive or cognitive clearness, we shall have to conclude from the two correlations that under our conditions the preëxposure eye-movements are *not* at all valid indicators of preëxposure set. It is not unlikely that a definite set for the right or left aroused by specific instructions from the experimenter would be accompanied by significant movements in the same direction. As support for this hypothesis, the results on ideomotor action may be cited. Without hetero-instructions as to right or left set, our *Os* probably obeyed the implicit instructions to be set equally for right and left sides. Any preëxposure eye-movements are therefore merely a phase of the general set and nervous tension preceding the expected exposure.

Extent of eye-movement and type of instruction. In addition to the fact that cognitive instructions resulted in an increased number of eye-movements, it occurred to us that the extent of the movements might also be similarly influenced. In this comparison we have limited ourselves to the very first movement following the exposure, since that seemed to be

[19] T. L. Kelley, *Statistical Method*, 1924, 268.

the most crucial movement of all. Table II gives the mean extents of the movements of every *O* under the two types of instruction in terms of millimeters of deviation on the record.

Table II shows that not only were there about 60% more eye-movements immediately following the exposures with cognitive instructions, but also the average increase in extent was about 26.5%. The only *O* who does not show a reasonable increase in extent of movement is *S*. In Table II is additional evidence of the greater ocular activity which is set up under the set to cognize objects.

Latent time of first eye-movement and type of instruction. The length of time elapsing between the moment of the exposure and the first eye-

TABLE III

NUMBERS OF FIRST EYE-MOVEMENTS TO THE RIGHT AND LEFT UNDER DIFFERENT INSTRUCTIONS

	Attributive set		Cognitive set		Both combined		
O	Left	Right	Left	Right	Left	Right	% Left
H	11	7	15	7	26	14	65.0
A	2	10	12	25	14	35	28.6
N	23	7	38	7	61	14	81.3
S	17	12	25	18	42	30	58.4
W	7	5	8	8	15	13	53.5
Totals	60	41	98	65	158	106	59.9
%	59.4	40.5	60.1	39.9	59.9	40.1	

movement is not very accurately determined due to various factors, some beyond our control. The uncertainty of our time-records has been mentioned before. The error involved in consequence of these faults of technique is probably not over 100σ, however. A greater error lies in the uncertainty as to which first eye-movements to include as significantly connected with the exposure. There were some records without any movements at all for the 4 or 5 sec. interval following the exposure. Other movements came perhaps 2 sec. later. Were the latter delayed reactions to the exposure or were they movements of a general character, in a class with those which preceded the exposure, for example? We arbitrarily selected all those movements occurring not later than 3 sec. after the exposure as having some connection with the observation. The means of the latent times for the various *O*s are given in Table II. If the latent times represented there may be given credence, then it is apparent that cognitive instructions reduce the reaction-time of the eye by about 40%. Only one *O* (*W*) seems to give an insignificant difference and that is based upon a very small number of cases.

The prevailing direction of the first eye-movement. It seemed of interest

to ask in what direction the eye is inclined to jump immediately after a tachistoscopic exposure. It is generally assumed that any *O* in this situation, with readable material, especially, takes a self-instruction to read and hence would tend to shift his fixation to the left through habit. Our technique yields data on this question. In Table III we give the numbers of first eye-movements to the left and right for every *O* and for the 5 *O*s combined.

Four of the 5 *O*s show some tendency for a movement to the left, as if under a set to read from left to right, in spite of the fact that the material was counterbalanced so as to give the right an equal advantage with the left. One *O* shows a very decided reversal of this rule, for some reason still unknown to us; this *O* is known to be fundamentally right handed. The general advantage for the right or left side held under both instructions for every *O*. In the combined result the mean preference for the left side was equally strong (about 60%) under both instructions. This suggests that the set-to-read was not entirely responsible for the lack of balance in the direction of eye-movements. Were the set-to-read an important factor, we should expect a much more unbalanced preference under the cognitive instructions than under attributive instructions. Such was not the case. It is an open question as to whether the preponderance of movements in one direction indicates a fundamental position factor for attention.

Preëxposure fixation and clearness. We found a few cases in which the fixation was definitely to the right or left of the center of the exposure-field at the beginning of the exposure. There were only 19 such cases under attributive instructions and 16 under cognitive instructions. A fixation to one side or the other might be expected to favor the side fixated, either for heightened attributive clearness or cognitive clearness or both. This should be true if foveal clearness is an important condition for either of the two types of clearness just mentioned. Contingency tables were made showing these relationships. Coefficients of contingency are not very reliable from so few cases. The tables indicate an actual *negative* correlation (—0.53) between foveal clearness and the consequent attributive clearness. When fixation was right the right side was clearer than the left in 1 case out of 10 and *less* clear in 6 cases, the remaining 3 cases having equal clearness on the two sides. When fixation was left, in 4 cases out of 9 clearness was greater on the right and in only 2 cases clearness was greater on the left. As for the effect of foveal clearness on cognition, the 8 fixations right and the 8 left both gave an even distribution of cognitive clearness; half the time the left had the greater cognitive clearness and half the time the right. These results are entirely too meager to justify any

dogmatic statements, but they do indicate that to a large extent both attributive and cognitive clearness are independent of foveal clearness.

Correlation between attributive and cognitive clearness. Dallenbach has postulated four possible categories involved in the relationship between attributive and cognitive clearness. An experience may be attributively clear and also cognitively clear, attributively clear and cognitively unclear, attributively unclear and cognitively clear, or attributively unclear and cognitively unclear. In our experiments we had no absolute judgments of either type of clearness, but we could easily note from *O*'s report which side of the exposed-field was clearer, either attributively or cognitively. We had a number of cases in which the right and left sides were equally clear, either attributively or cognitively. Cognitive clearness was always indicated by the fullness of report of objects. We could not include in this classification of cognitive clearness the slides depending upon the factor of isolation, however, for the two sides had an unequal amount of material which could be reported. We limited the comparison to the observations made under attributive instructions, since in these we were always sure to receive a report of both types, whereas under the cognitive instructions a report of attributive clearness was not always certain or forthcoming. If we pool the observations of 5 *Os*, and they were all quite similar, we have 180 cases to correlate. A 9-fold table was constructed and a coefficient of contingency was computed. This was then translated into an equivalent Pearson r as was done above. The correlation between the two types of clearness thus obtained is +0.539. This is surely sufficient to be indicative of a positive relationship and yet low enough to indicate much independence. The coefficient of alienation is 0.84.

The correlation may be obtained in still another manner. Suppose that we treat every item or object on the slides as a single case. Let us say that any item correctly reported is cognitively clear and any item not reported is cognitively unclear. We also know from *O*'s drawings whether the named or unnamed objects lay within the attensively clear or unclear regions of the field. We can prepare a contingency table with Dallenbach's four categories. Pooling the results from all *Os* again, we find 2275 items that can be so classified. Only 345 of these were cognitively clear, the remaining 1930 being unclear. Under the attributive instructions this is what we might expect. As to attributive clearness, 1224 of the items lay within the clearest portion of the fields and 1051 were at a lower level. The correlation is +0.311. This is somewhat lower than the one found by the other method, but it is quite similar to it. The coefficient of alienation is 0.95, demonstrating again the relative independence of the two types of

clearness. It is quite possible, of course, that the differences in attributive clearness are entirely too small to cause much of a corresponding difference in cognition. Certainly things so attributively unclear that they are not even noticed are rarely if ever cognized. But granting more than a certain minimum level of attributive clearness, an increase in attributive clearness may mean little or nothing for cognition. The differences with which we are dealing may have been merely the 'wrinkles' within the upper level of attributive clearness and not at all the sharp break that more probably exists between the focus and the margin in attentive consciousness. We shall come back to this problem in connection with the discussion of levels.

Relation of significant post-exposure eye-movement to clearness. A 9-fold contingency table was prepared showing the relationship between the direction of the significant eye-movement after the exposure and the side of the field reported with greater attributive clearness. The correlation in this case was +0.215, being based upon 240 observations. A similar correlation based upon 180 observations (the isolation slide being omitted) between direction of eye-movement and cognitive clearness was +0.331. These very low correlations were a decided blow to the hope of using the direction of the first significant eye-movement after the exposure as an objective indicator of the more attention-compelling side of the field. Whether we use attributive or cognitive reports the result is approximately the same. It might be objected that a momentary exposure hardly has more than a passing influence upon fixation, and, that if the material were left in view longer, the first eye-movement and the duration of fixation on either side might be more closely correlated with clearness. From another set of experiments, which will not be reported here, we found but slightly higher correlations between cognitive clearness and the first significant eye-movement; they were +0.468 and +0.398 for the 'size' and 'novelty' slides respectively.

Relation of the factors of attention to clearness. In this comparison 4-fold tables were made, noting whether the stronger factor for attention were right or left and whether the greater attensity (or cognition) were also right or left. All cases of equal attensity or equal cognition were eliminated from consideration. Here we find very positive results. The factors of attention proved to be effective upon attensity to the extent $r = +0.678$ and upon cognition to the extent of $r = +0.820$. One could decide from these correlations, either that the factors are more potent in determining cognition than in determining attributive clearness, or that the cognitive report is a more valid criterion of the relative effectiveness of two competing stimulations. Since the attributive clearness is the more immediate effect in

time and the less complicated response, we might expect it to reflect most clearly the potency of factors in the stimulus. This being reasonable it follows that of the two conclusions given above the second is more nearly correct; that the cognitive report is more valid than the attributive.

The factors of attention and significant eye-movements. The very small amount of agreement between post-exposure eye-movements and either attributive or cognitive clearness that was demonstrated above led us to doubt whether eye-movements could be used at all as indicators of attention values of stimuli. This is verified when we correlate the relative potency of the two sides of a slide with the direction of the first significant movement. In the attributive series of observations the correlation was +0.050 and in the cognitive series it was +0.124. These correlations are insignificant. Again, in the momentary exposures, at least, neither side of our slides had sufficiently greater potency to induce an eye-movement in that direction consistently. When the exposure was made 10 sec. long, in some other experiments, the correlations between the factors of attention and first fixation were +0.373, +0.345, and +0.582 for the 'isolation,' 'size,' and 'novelty' slides respectively. These coefficients agree very closely with those between eye-movement and cognitive clearness for longer-than-momentary exposures which were cited above. Lengthening the exposure does therefore give eye-movements some small validity as indicators of attention value of stimuli and of the degree of cognition, but certainly not sufficient validity for practical purposes.

Correlations when other variables are held constant. The various coefficients of correlation which have been given in different connections are summarized in the first column of Table IV. The numbers of cases upon which they are based appear in column two. In column three are given the first order partial correlations and in the last column the second order correlations. These are presented with some hesitation, since the original data were tabulated in crude categories and the Pearson r's were estimated from coefficients of contingency. Furthermore, we were not able to include exactly the same sets of observations in finding the different raw correlations. We have to assume that the correlations represent the real abstract variables for which they are named, *i.e.* clearness, attention values of stimuli etc., regardless of the particular observations from which they are derived. While some of the partial correlations are without any real meaning others seem highly significant.

Interpretation of the partial correlations. The raw correlation between attributive and cognitive clearness (+0.539) is low enough to reveal their true independence. The partial, $r_{ac.e}$ means the same correlation with post-

exposure eye-movements held constant. This has very little meaning since the eye-movements could hardly be expected to exert any causal effect on the preceding clearness. The partial $r_{ac.f}$, between the two kinds of clearness with the factors of attention held constant, is very enlightening. It is practically zero. In other words, neither type of clearness can be said to be the cause of the other although both are determined by the factors of attention as shown by the raw correlations r_{af} and r_{cf}. Each type of clearness would seem therefore to be independently determined by the factors in the stimulus. This may not be a general phenomenon. As was pointed out before, some degree of attributive clearness is essential for

TABLE IV

SUMMARY OF THE COEFFICIENTS OF CORRELATION

a=attributive clearness; c=cognitive clearness; e=eye-movements (after the exposure); f=factors of attention.

Zero order	No.	First order	Second order
$r_{ae}=.539$	180	$r_{ae.e}=.508$	$r_{ae.ef}=-.071$
		$r_{ae.f}=-.051$	
$r_{ae}=.215$	240	$r_{ae.f}=.246$	$r_{ae.ef}=.333$
		$r_{ae.e}=.046$	
$r_{ce}=.331$	180	$r_{ce.a}=.261$	$r_{ce.af}=.596$
		$r_{ce.f}=.565$	
$r_{af}=.678$	159	$r_{af.e}=.489$	$r_{af.ce}=.521$
		$r_{af.e}=.683$	
$r_{cf}=.820$	107	$r_{cf.e}=.790$	$r_{cf.ea}=.704$
		$r_{cf.a}=.750$	
$r_{cf}=.054$	189	$r_{cf.e}=-.272$	$r_{cf.ea}=-.286$
		$r_{cf.a}=-.131$	

cognition. The causal independence indicated by $r_{ac.f}$ can be maintained at present only for the conditions of this study.

The coefficient r_{ae} indicates a very slight tendency for the eyes to move in the direction of greater attributive clearness. This tendency is probably not changed if the external factors are kept constant, as indicated by $r_{ae.f}$, but it is reduced to zero when the cognitive efficiency is equal for the two sides, indicating that the eyes actually *follow* the path of clearest cognition! A very astonishing result!

The eyes, similarly, tend to follow the area of good cognition, as indicated by r_{ce}. When attensity is held constant for the two sides of the field this tendency remains about equally strong but for equal external factors the tendency is noticeably increased (see $r_{ce.f} = +0.565$), a further verification of the above conclusion.

The two kinds of clearness were found to depend very strongly upon the factors of attention, as was shown by the raw coefficients. Holding either

one constant, however, tends to reduce this relationship slightly. The correlation r_{af} of $+0.678$ is reduced to $+0.489$ in the partial $r_{af.c}$. The correlation of r_{cf} of $+0.820$ is reduced to $+0.750$ in the partial $r_{cf.a}$. This would seem to indicate some fundamental causal relationship between the two kinds of clearness that we failed to find in the partials $r_{ac.f}$ and $r_{ac.ef}$.

In view of the zero correlation between the factors of attention and the consequent eye-movement it is a little surprising to find this raised to $-.272$ in the partial $r_{ef.c}$ and $-.286$ in the partial $r_{ef.ca}$. This would seem to imply that if cognition were equal on the two sides of the field, the eyes would respond *negatively* more often than positively to the factor of

TABLE V

FREQUENCY OF THE VARIOUS NUMBERS OF LEVELS OF CLEARNESS

O	One	Two	Three	Four
H	0	36	12	0
N	1	34	12	1
A	5	29	14	0
S	3	20	23	2
W	1	34	13	0
Totals	10	153	74	3
%	4.6	63.9	30.8	1.2

attention. Why this should be so is hard to understand. The coefficients are too low to trouble us long but sufficiently suggestive to stimulate us to study this point further.

Levels of attributive clearness. The 240 observations under attributive instructions were classified according to the number of different levels of clearness reported by the various *O*s. The results are given in Table V.

It will be seen that the 5 *O*s are extremely consistent in the numbers of cases of each number of levels. The exposure-field falls into two levels in about 64% of the cases and three levels in about 31% of the cases. A two-level report typically means one area of heightened clearness, the remainder of the field being at a lower level. In a three-level report one side of the field would be reported as most prominent, the other side less prominent and a third area, such as the margin, is present but less clear or prominent than the rest. It made no difference what type of slide was exposed, the proportions of one, two, three and four level reports were the same.

It might be maintained that, when more than two levels are reported, *O* is really experiencing successive patterns of clearness. When questioned about this point the *O*s typically asserted that the multi-level experiences were simultaneous. There is still the possibility of a time illusion; what is

actually successive seems to *O* simultaneous. We know of no way at present of settling this point.

It might be maintained that the additional areas of clearness are merely wrinkles in the upper level. This can hardly be correct since, in addition to the numbers of levels already displayed in Table V, there were reports of 'wrinkles' of clearness both in the areas of greater clearness and in the areas of lesser clearness. There were altogether 74 reports of such 'wrinkles' and every *O* reported at least 12, which shows that every *O* distinguishes 'wrinkles' from levels. By far the greater number of these 74 'wrinkles' (58, or 78%) were observed within the higher level of clearness.

A proposed theory of levels of clearness. In view of the above results we are ready to propose the following hypothesis concerning levels of attributive clearness. We prefer to speak of these levels fundamentally as levels of nervous functioning. Even Titchener has asserted that in general the conditions or determining factors for attributive clearness are those things that by their nature have a powerful effect upon the nervous system.[20] The lowest level of nervous functioning is entirely unconscious; attributive clearness is zero. The second level is the obscure conscious level (the lower level of Titchener's theory). Experiences of this level are usually not reportable, and are reportable at all only by retrospection. These experiences belong to the unfavored senses; the ones not being used at the moment for observing purposes or for active dealing with the environment. Habitually we favor the eye and the ear. Somesthetic sensations and affective processes customarily remain at the second, obscure level. The set or adjustment typically found in attention usually favors one sense at a time; we look or we listen; we are eye-minded or ear-minded for the moment. We may look and listen simultaneously, apparently, but are the visual and auditory experiences together in the focal level of clearness, is there a shifting back and forth between them, or are the two at different levels of clearness above the second (obscure but conscious) level which has just been described? Most experimental arrangements fail to yield an answer to this question; they deal with one sense field at a time.

When one sense field is the object of study, as in the present investigation, more than one level of clearness within that sense department has been the usual report. This, we take it, is the traditional 'two-level' report. In drawing conclusions about the number of levels of clearness in the field of consciousness taken as a whole, the other senses are for some reason usually overlooked. If the experiment is on visual attention, does the lower

[20] *Feeling and Attention*, 204.

(visual) level reported also extend to and include the auditory, kinesthetic and other experiences of the moment, and are they all on a par with the visual as to degree of clearness? We very much doubt this, although in rare cases of very great absorption in one detailed object it could perhaps happen. It is our opinion that the favored sense may have one or more levels of clearness, typically two, above the two already described. This opinion is based upon the facts of Table V. This would make the *typical* conscious field a three-level affair. The lowest or third level is inferred rather than described, but by its very nature it is overlooked by *O* in the ordinary observation. *O* does not report it spontaneously but when cross questioned about it he can often describe it by retrospection. Above this, within the favored sense, there are usually two, and sometimes three distinct levels, with uneven divisions or wrinkles of clearness within them. These levels are typical of the tachistoscopic situation. Whether they are typical also of everyday experience is of course another question.

Summary and Conclusions

Forty-eight exposure fields were presented before the eyes of 5 *O*s under each of two different sets of instructions: (1) attributive (to describe the division of the field as to attributive clearness); and (2) cognitive (to name or describe the objects therein), for a period of 0.1 sec. A record of the saccadic eye-movements of the *O*s was obtained by the Dodge photographic technique. The significant results were as follows:

(1) A surprisingly large number of eye-movements occur before as well as after a tachistoscopic exposure in spite of efforts to fixate a point before the exposure. There are even more movements in the 3 sec. just before the exposure than in the 3 sec. just after the exposure.

(2) The cognitive instructions induce about 14% more movements before the exposure and about 100% more following the exposure as compared with the attributive instructions.

(3) The direction of the eye-movement before the exposure is not at all indicative of the side of greater subsequent clearness, either attributive or cognitive. This is in the absence of any specific instruction for *O* to give his attention right or left.

(4) The cognitive instructions induce about 60% more eye-movements immediately after the exposure, with an average extent about 27% greater, and with a latent time about 40% shorter than do the attributive instructions.

(5) Of the first post-exposure eye-movements, about 60% are toward the left. One *O*, however, showed a decided reversal of this rule.

(6) The correlation between the clearer side of the field for cognition and the clearer side for attensity is probably not greater than +0.50 and when the common factor for attention is held constant by partial correlation this reduces to zero, indicating a real independence of attributive and cognitive clearness.

(7) The correlation between the side of greater clearness and the direction of the first post-exposure eye-movement is probably not greater than +0.40, but it is higher in the case of cognition than it is for attributive clearness.

(8) The factors of size, novelty, isolation, and position are effective for both attributive and cognitive clearness, but more for the latter than for the former as indicated by correlations of about +0.68 and +0.82.

(9) The same factors of attention are almost entirely ineffective in determining eye-movements after momentary exposures and they are only slightly effective in longer exposures, with the type of field employed.

(10) The typical report of attributive clearness finds two levels within the exposure field (64% of all reports). The next most popular report was of three levels (31%). As few as one and as many as four were observed in a 0.1 sec. exposure. A three-level theory of the typical consciousness during tachistoscopic exposure is proposed.

CONTRAST EFFECTS AND THE PSYCHOPHYSICAL JUDGMENTS

By MALCOLM G. PRESTON, University of Pennsylvania

Fernberger,[1] in an experiment with lifted weights, reported that comparison stimuli, whose values lie within the interval of uncertainty and which occur immediately after the lightest comparison stimulus of the series, will most probably be judged heavier, while comparison stimuli, whose values lie within the interval of uncertainty and which occur immediately after the heaviest comparison stimulus of the series, will most probably be judged lighter. In the discussion of this fact Fernberger did not decide whether the increased probability of judgment should be attributed to stimulus-factors or to a tendency for the observer to avoid the repetition of judgments.

That the presence of an extreme stimulus in the immediate neighborhood of a pair to be judged influences the judgment of the pair to be judged was confirmed by Guilford and Park who showed a decrease in the number of greater judgments when a 400-gm. weight was interpolated between standard and comparison weights ranging from 185 to 215 gm. in weight, and an increase in the number of greater judgments when a 100-gm. weight was so interpolated.[2]

In an experiment which was designed to use the psychophysical judgments as a medium for the study of the Gibson-Wulf problem of changes in retained visual material, Turner showed that under certain conditions both the threshold for *greater* and the threshold for *lighter* computed on judgments made after an immediately preceding judgment of *lighter* will be increased, indicating a decrease in the *greater* judgments and an increase in the *lighter* judgments in the series under consideration.[3] On the basis of this evidence together with the remaining evidence of his experiment, Turner concluded that standard weights tend to shift their effectiveness toward the effective value of the preceding weight. According to this theory a standard weight, following a pair of which the second is the lightest of the series, will shift in effective value towards the lighter end of the continuum, consequently giving a greater probability that its comparison weight will be judged heavier. Likewise a standard weight preceded by a heavy weight, will shift to the heavy end of the continuum giving a greater probability for the succeeding comparison weight to be judged lighter. Therefore Fernberger's contrast effect seems to be explained as an

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[1] S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol*, 3, 1920, 126-150.

[2] J. P. Guilford and D. G. Park, The effect of interpolated weights upon comparative judgments, this JOURNAL, 43, 1931, 589-599.

[3] W. D. Turner, Intra-serial effects with lifted weights, this JOURNAL, 43, 1931, 1-25.

immediate result of the operation of the factors involved in Turner's theory of the shifting of the effective value of the standard according to the value of the preceding weight. In a footnote Turner remarks "one can easily conclude that the 'intra-serial effects with the method of constant stimuli' are explicable by reference to the weight and not to the judgment which precedes a particular comparison."[4]

This conclusion is not entirely substantiated by later experimentation reported by Arons and Irwin.[5] These authors report that when an *S* is presented with a series of equal weights of 100 gm., the mathematical expectancy for the immediate repetition of any given category of judgment is greater than the number of repetitions found by counting the sequences. This effect is particularly noticeable in the case of the *equal* judgments, and, although the statistical reliability of some of their results is low, they feel that added significance attaches to them by virtue of the uniformity of findings which obtained throughout all series and all *S*s. Arons and Irwin do not claim that their results necessarily conflict with those reported by Turner; they write, however, that the methods used in his investigation were not adequate for showing an effect of previous comparison which might be present in the form of a constant factor.

Despite this statement there are certain of their results which should be explicable by the theory of the fluctuation of the effective value of the standard but which are not immediately explicable by this principle. Under the conditions of the experiment which Arons and Irwin report, the immediately preceding comparison weight differs not at all from the succeeding standard stimulus. Consequently, with the exception of influences arising from the time errors, no reason exists for any great difference between the mathematical expectancy for and the found number of repeated judgments. A chance distribution of variations around the mathematical expectancy of repeated judgments would be expected. It may be argued that the variation of repetitions of the judgments *greater* and *lighter* from their respective mathematical expectancies, which Arons and Irwin show, is in accordance with this reasoning. These judgment sequences are obviously affected by the positive and the negative time-errors, developed by the comparison on the first pair of each sequence which would be shown in the effective value of the comparison weight of the first pair. This fact fails, however, to explain the pronounced tendency towards the avoidance of the repetition of *equal* judgments in which no time-error can be ascribed to the judgment on the first pair which Arons and Irwin's results indicate. Granting the argument just reviewed based on time-errors, a judgment of *greater* would, by Turner's theory, be followed most probably by a judgment of *lighter*, while a judgment of *lighter* would be followed most probably by a judgment of *greater*. No reason exists for expecting any preponderance of *lighter* or *greater* judgments after a judgment of *equal*.

A second fact upon which evidence is only partly at hand concerns the frequency, relative to the mathematical expectation, of the various possible combinations of judgment-categories in sequence, apart from repetitions, encountered with equal weights. Arons and Irwin's results are not clearly defined on this point. They find the most probable judgment after *greater* to be *equal*, the most probable after *lighter* to be *greater*, and after *equal* they find little or no difference between the frequencies of *lighter* and *greater*.

[4] Turner, *op. cit.*, 16.

[5] L. Arons and F. W. Irwin, Equal weights and psychophysical judgments, *J. Exper. Psychol.*, 15, 1932, 733-751.

A further argument dealing with this general problem may be made on the grounds that the time-error involved in the judging of the second pair composing each sequence may account for the deviation of the Arons-Irwin results in the case of sequences whose first judgment is *greater* from the results as expected by the theory of the shifting effective value of the standard. Although Arons and Irwin show that an effect of overestimation of the second weight obtains under the conditions of their experiment, this fact cannot be admitted to account for the deviation in the observed number of any given judgment sequence. The effect of overestimation had already been reflected in their computation of the most probable number of judgment sequences from which the observed number deviates, and the argument is therefore of no moment.

Because of the tendency to avoid *equal* judgments it would be extremely difficult to reconcile completely the Arons-Irwin results with Turner's theory. According to the theory no reason could be given to explain the deviation of the found from the expected number of repeated *equal* judgments. This conclusion with respect to *equal* judgments cannot, however, be applied legitimately to the effect as it relates to the *lighter* and *greater* judgments, since the procedure used by these investigators and the results therefrom conform in every important respect to Turner's requirements. In every event, except where *equal* judgments are repeated, it appears that at present Turner's theory can be used to explain the Arons-Irwin results. On the other hand, the intra-serial effects may very well arise from more than one source and the identification just proposed may be more apparent than real.

The present experiment was undertaken to explore more thoroughly the factors affecting the non-repetition of judgments on equal weights and their relation to the general problem of contrast effects. In addition to the type of evidence contained in the report of Arons and Irwin's experiment, certain other facts suggest themselves as pertinent to the study of the relation of Turner's results to those of Arons and Irwin.

As Arons and Irwin have suggested, the effect of previous comparisons may show not only in the diminished or increased number of judgment sequences whose members are immediately adjacent to each other, but also in the case of judgment sequences whose members are separated by intervening judgments. In the case of a large number of judgment sequences of a given kind, for example the sequence of *lighter* judgments followed by *lighter* judgments, in which the sequence is constructed with an undetermined judgment interposed between the first and last judgment of the sequence, a deviation of the found number of sequences from the expected number could not be explained by Turner's theory unless the theory were modified considerably. Under these conditions the theory as it stands would have to account for differences between found and expected values where the intermediate judgments have been uncontrolled as to category and the stimulus values are equal. As a consequence of the conditions of this method of selecting the sequences the intervening judgments could have no effect upon the distribution of judgment sequences under consideration.

PROCEDURE

Series A, the only series used in this experiment, consisted of 14 pairs of weights objectively equal, each weight being of cast aluminum and surrounded on the lifting surface by a felt pad. Each weight including the pad weighed 100 gm. The weights were presented to *S* at a rate of 92 beats per min. They were lifted consecutively in rhythm with an electrically driven metronome from a freely moving turn-table.

A screen separated *S* from the turn-table. In a sitting 324 pairs of weights were lifted; each sitting was divided into six periods during which 54 pairs of weights were lifted and compared.

All weights were checked on a chemical balance before, during, and after the experimentation and were found to be constant within the limits of 10 mgm. Experimentation was begun in May, 1933, was concluded in June, 1934, and was carried on in the Psychological Laboratory at the University of Pennsylvania.

The instructions to *S* were: "I am going to present you with a series of pairs of weights, the second of which is either equal to or varies slightly in weight from the first. Please report whether the second of each pair is lighter than, equal to, or heavier than the first."

Ss in this experiment were: Esther Israeli (*Is*), graduate student in psychology; Wendell R. Carlson (*Ca*), member of the staff of the Department of Psychology; Milton W. Mays (*M*), Harrison Fellow in Economics; John F. Slack (*Sl*) and Arthur G. Rohs (*R*), undergraduates in the Wharton School. Of the *Ss* only *Ca* had had any experience in lifting weights.

During the same period in which the experimentation reported herein was being carried through, additional experiments were being made with the same *Ss*, experiments which dealt with other problems related to the contrast effects. The distribution of experimentation in each case was such that each sitting which contributed data used in this report was preceded on the previous day by a sitting with a series which on some occasions was composed of pairs of unequal weights and on other occasions of pairs of equal weights. A total of approximately 90,000 judgments was made by 5 *Ss* during the period of the year in which the experiment was being carried on, and of these judgments 8748 were used as data for the present study.

The ratio of the difference between the expected and the found number of judgment sequences to the standard error of the expected frequency, referred to hereafter as Diff./σ freq., was computed by the method given by Arons and Irwin[6] which involves the formula $\sigma \text{ freq.} = (pqn)^{\frac{1}{2}}$. In the presentation of the data of this study a degree of choice is permissible among the kinds of values which may be considered to bear upon the problem at hand. Although it is true that a large value of Diff./σ freq. may refer as well to a small difference as a large one, the problem treated here does not depend for its solution upon the magnitude of the difference as much as it depends upon its reliability and its positive or negative character. Consequently, we have preferred to rest the argument upon the sign of the difference value, upon the magnitude of the value Diff./σ freq., as a criterion of reliability, and upon the number of positive and negative differences encountered in the various sittings of each of the *Ss*, as a further criterion of reliability.

Results

The total number of sittings and judgments made by each *S* in connection with the data used in this study are as follows:

Ss	*Is*	*Ca*	*M*	*Sl*	*R*
Sittings	4	5	6	6	6
Judgments	1296	1620	1944	1944	1944

[6] Arons and Irwin, *op. cit.*, 740 f.

TABLE Ia

SHOWING RATIOS OF DIFFERENCE BETWEEN EXPECTED AND OBSERVED FREQUENCIES OF JUDGMENT SEQUENCES TO STANDARD ERROR OF EXPECTED FREQUENCIES IN THE CASE OF SEQUENCES WITH NO INTERVENING JUDGMENTS

O	Seq.	Sitting 1	2	3	4	5	6	Cons.
	LL	−1.26	− .77	− .53	− .47	− .03		− .41
	LE	.02	.51	− .26	− .06	− .49		− .43
	LG	.99	.36	.94	.56	.37		.82
	EL	1.69	− .19	1.55	.78	.84		1.80
Ca	EE	−2.51	−2.74	−2.95	−2.99	−1.93		−5.76
	EG	.69	2.14	.30	1.46	.65		2.52
	GL	− .35	.53	− .81	.09	− .56		−1.10
	GE	1.60	1.60	2.80	1.95	1.69		4.45
	GG	−1.05	−1.78	−1.32	−1.61	− .79		−2.40
	LL	− .07	−1.10	− .56	−2.03			−1.18
	LE	− .58	−1.02	.04	1.07			− .23
	LG	.22	1.20	.67	1.04			1.11
	EL	− .58	− .41	.68	.79			.26
Is	EE	− .60	−1.57	− .60	−2.07			−2.55
	EG	.60	.72	− .42	.55			.82
	GL	.36	.51	− .06	1.34			.66
	GE	.60	1.35	.42	.33			1.42
	GG	− .56	−1.14	− .22	−1.27			−1.29
	LL	− .13	− .22	− .02	− .44	.08	−2.20	− .80*
	LE	− .63	.74	− .85	×	×	×	− .67*
	LG	.69	− .08	.44	.55	− .36	1.92	.67*
	EL	.71	− .32	− .85	×	×	×	−1.34*
M	EE	− .64	.13	− .78	×	×	×	−1.16*
	EG	.19	.00	.45	×	×	×	.68*
	GL	− .55	.47	.46	.97	.17	2.31	1.53*
	GE	1.21	− .83	.79	×	×	×	.41*
	GG	− .79	− .12	− .64	− .75	.09	−2.04	−1.28*
	LL	.04	.83	− .49	.46	−1.23	−1.99	− .34
	LE	− .45	.26	− .31	−1.46	− .93	− .23	−1.55
	LG	.21	− .58	.71	.86	1.53	1.21	1.38
	EL	.88	.80	1.86	.24	− .24	1.06	1.63
Sl	EE	− .98	−1.79	−2.15	−4.14	−4.69	−5.98	−8.24
	EG	.35	1.00	.62	3.30	4.56	4.98	6.13
	GL	− .43	−1.25	−1.43	− .37	.89	.47	−1.29
	GE	1.12	1.60	2.30	4.70	4.42	5.27	7.97
	GG	− .54	− .60	−1.08	−3.68	−4.61	−5.14	−6.17
	LL	−1.87	.00	×	− .78	−1.30	−1.29	− .90
	LE	.23	− .18	×	1.16	1.78	.72	.40
	LG	.91	.52	×	−1.38	−1.14	.27	− .08
	EL	1.69	.73	×	.80	.28	1.22	.62
R	EE	−1.48	−1.47	−2.56	−2.19	−2.82	−3.09	−4.37
	EG	.53	1.60	2.70	2.22	2.87	2.94	5.3
	GL	− .20	− .78	×	− .85	.38	− .53	− .48
	GE	1.16	1.60	2.16	2.50	2.10	3.24	5.07
	GG	−1.14	−1.91	−2.27	−2.75	−2.62	−3.23	−5.62

*Includes only Sittings 2-6

TABLE Ib

SAME AS TABLE Ia EXCEPT THAT MEMBERS ARE SEPARATED BY A SINGLE INTERVENING JUDGMENT

O	Seq.	Sitting 1	2	3	4	5	6	Cons.
	LL	− .71	−1.64	− .13	− .12	−1.02		− .52
	LE	.34	.59	1.01	.92	.47		1.17
	LG	.49	.98	− .54	− .31	.68		− .14
	EL	.10	1.99	.60	− .15	.24		.87
Ca	EE	−1.20	−1.96	−1.93	− .15	−1.87		−3.72
	EG	.76	.04	.90	1.70	1.35		2.22
	GL	.32	− .37	− .22	.00	.84		− .35
	GE	.15	.78	.21	− .24	.72		1.02
	GG	− .61	− .54	.11	− .34	−1.44		− .87
	LL	− .74	−1.41	−1.50	−1.06			−1.67
	LE	.79	1.19	1.03	.88			1.72
	LG	.22	.34	.96	.33			.58
	EL	− .93	.42	− .75	.88			− .23
Is	EE	− .60	−1.67	− .60	−1.57			−2.27
	EG	.88	.17	.79	.47			1.06
	GL	1.22	.51	− .06	.33			.58
	GE	− .54	.38	− .34	− .20			− .14
	GG	− .69	− .44	.22	− .37			− .38
	LL	− .13	.55	− .17	−1.18	− .54	− .94	− .37*
	LE	.13	− .32	.74	×	×	×	− .34*
	LG	− .16	− .36	− .06	.46	.04	.97	.06*
	EL	1.09	.21	− .32	×	×	×	− .67*
M	EE	−1.71	.13	− .78	×	×	×	−1.16*
	EG	.81	− .30	.10	×	×	×	.14*
	GL	− .94	− .36	.09	1.44	.97	.84	.92*
	GE	1.15	− .30	− .24	×	×	×	− .14*
	GG	− .36	.34	.02	− .86	− .48	− .88	− .48*
	LL	1.58	− .52	−1.02	.57	−1.10	.27	.51
	LE	−1.26	.18	− .45	−1.56	.56	− .16	−1.37
	LG	.00	.02	1.07	1.00	− .06	.19	1.02
	EL	.88	.18	.97	.15	1.26	.37	1.31
Sl	EE	1.10	−1.44	−2.19	− .13	.05	2.21	− .04
	EG	−1.38	1.04	1.12	.16	− .25	−2.15	− .67
	GL	−1.49	.24	.21	− .43	−1.02	− .52	−1.20
	GE	− .15	.89	2.67	1.11	− .39	−2.01	.80
	GG	1.06	− .78	−2.24	− .66	.40	2.12	.00
	LL	.27	.00	×	− .78	−1.67	− .83	.95
	LE	.65	− .18	×	.07	− .09	.24	− .62
	LG	− .13	.52	×	.21	1.39	.27	.19
	EL	.86	.73	×	− .29	2.07	.00	.49
R	EE	−1.48	− .90	−1.12	.27	−1.01	−1.02	−1.42
	EG	1.16	1.15	1.26	− .37	− .40	1.03	1.53
	GL	− .86	− .78	×	.74	−1.39	.27	−1.02
	GE	.85	.88	1.53	− .09	1.29	.88	2.12
	GG	− .15	−1.21	−1.79	.18	− .53	−1.01	−1.94

* Includes only Sittings 2–6.

TABLE Ic

SAME AS TABLE Ia EXCEPT THAT MEMBERS ARE SEPARATED BY TWO INTERVENING JUDGMENTS

O	Seq.	Sitting 1	2	3	4	5	6	Cons.
	LL	− .93	.76	−1.50	.11	− .13		.00
	LE	− .45	.26	.54	.92	.24		.39
	LG	1.27	− .37	1.25	− .61	− .14		.04
	EL	1.45	.50	− .26	.49	−1.09		.10
Ca	EE	−2.29	− .38	− .81	− .14	.99		−1.18
	EG	.51	.22	.83	.00	.78		1.15
	GL	− .42	−1.22	1.57	− .61	.95		− .39
	GE	.20	.04	.14	− .97	−1.12		.32
	GG	−1.22	.47	−1.81	.86	− .28		− .50
	LL	.08	− .47	− .20	1.64			.79
	LE	1.26	1.19	− .29	− .97			.10
	LG	− .50	− .17	.48	− .95			− .84
	EL	− .49	.42	1.52	− .42			.10
Is	EE	−1.34	−1.19	−1.34	.97			− .89
	EG	.63	− .04	− .42	.24			.10
	GL	.36	.00	− .68	−1.07			− .99
	GE	− .75	.17	.70	− .20			.02
	GG	.00	.02	.04	.78			.74
	LL	.31	− .62	− .89	− .08	− .03	1.26	.62*
	LE	− .58	−1.38	.21	×	×	×	−1.34*
	LG	.24	.89	.49	− .06	− .49	−1.30	− .77*
	EL	− .19	− .85	1.80	×	×	×	.00*
M	EE	− .12	4.91	− .78	×	×	×	.00*
	EG	.54	− .30	− .93	×	×	×	− .96*
	GL	.04	.93	.19	.50	.58	−1.16	− .09*
	GE	.31	− .30	.10	×	×	×	.14*
	GG	− .59	− .75	− .07	− .19	− .05	1.20	.43*
	LL	.04	.28	1.36	1.23	−1.10	−1.48	.84
	LE	.08	− .94	− .87	− .35	2.10	.60	− .19
	LG	− .21	.54	− .29	− .49	− .85	.66	− .28
	EL	−1.26	− .42	− .63	− .84	−1.10	.10	−1.80
Sl	EE	.15	.50	.68	2.16	− .07	−1.02	1.07
	EG	.61	− .36	− .06	−1.19	.50	.97	.13
	GL	1.06	.31	.36	− .08	.79	.66	1.11
	GE	− .31	.24	− .68	−1.66	− .62	.52	−1.09
	GG	− .44	− .19	.38	1.41	− .01	−1.02	.09
	LL	.58	.00	×	− .78	1.16	−1.67	2.80
	LE	.08	.73	×	.07	.06	.64	− .62
	LG	− .64	− .78	×	.05	− .84	.53	− .48
	EL	.09	− .18	×	.80	.06	.89	− .18
R	EE	− .81	.55	.36	.13	.55	.53	1.19
	EG	.80	− .22	− .19	− .46	− .66	− .93	− .81
	GL	− .86	.52	×	− .97	−1.09	− .27	−1.29
	GE	.48	−1.04	− .60	− .17	− .51	−1.07	−1.34
	GG	− .13	.97	.45	.58	1.07	1.00	1.60

* Includes only Sittings 2–6.

TABLE Id

SAME AS TABLE Ia EXCEPT THAT MEMBERS ARE SEPARATED BY THREE INTERVENING JUDGMENTS

O	Seq.	Sitting 1	2	3	4	5	6	Cons.
	LL	.70	−1.42	−1.24	.38	− .51		− .04
	LE	− .45	.90	1.55	− .95	.24		.29
	LG	− .05	.92	.30	.52	.17		.25
	EL	− .69	.66	.14	− .95	.47		− .49
Ca	EE	.28	−2.02	−1.18	.66	− .33		−1.15
	EG	.51	1.00	.52	1.22	.08		1.70
	GL	− .14	.07	.78	− .09	.17		− .18
	GE	.15	.64	− .62	.49	− .34		.41
	GG	− .20	−1.01	− .43	−1.00	− .09		− .84
	LL	− .07	− .03	.34	− .06			.67
	LE	.21	− .35	.07	− .50			− .55
	LG	− .01	.07	− .10	.18			− .33
	EL	2.31	.04	−1.74	.33			.26
Is	EE	−1.34	− .10	.90	−1.57			− .87
	EG	−1.25	− .19	.97	.91			.36
	GL	− .83	.07	.19	.03			− .63
	GE	.20	.44	.42	.68			.61
	GG	.65	− .11	− .21	− .55			.17
	LL	− .49	− .45	.99	− .60	− .40	−1.37	− .33*
	LE	− .17	− .32	.21	×	×	×	− .67*
	LG	.24	.46	− .88	.08	− .09	1.24	.09*
	EL	.40	.74	1.27	×	×	×	.67*
M	EE	−1.92	.13	− .78	×	×	×	−1.16*
	EG	1.70	− .72	− .84	×	×	×	− .96*
	GL	.24	.33	− .88	.78	.84	1.38	.52*
	GE	1.89	− .30	.21	×	×	×	.14*
	GG	−2.14	− .27	.81	− .35	− .35	−1.25	− .22*
	LL	− .45	− .24	−1.10	1.31	.73	1.13	.84
	LE	− .45	− .75	1.54	.47	−1.01	− .16	− .19
	LG	.29	.47	− .97	− .97	1.38	− .35	− .28
	EL	.35	− .23	2.26	− .52	− .29	− .94	.13
Sl	EE	− .52	−1.28	−1.36	−1.93	1.00	.42	−1.12
	EG	.60	1.44	− .02	2.05	− .62	.37	1.29
	GL	.29	.47	− .97	− .58	− .85	.12	− .55
	GE	.29	1.80	− .02	1.27	− .76	− .52	.74
	GG	− .55	−1.54	.65	− .86	.54	.14	− .68
	LL	− .47	.00	×	.52	.07	− .18	2.01
	LE	.78	− .18	×	.43	− .58	− .59	−1.37
	LG	− .29	.11	×	− .74	.40	.97	.46
	EL	.78	− .18	×	.80	− .45	− .84	−1.37
R	EE	− .31	.41	− .34	− .81	− .49	.78	.26
	EG	− .23	.11	.72	.34	1.03	− .10	.88
	GL	− .74	.11	×	−1.38	.40	.97	.05
	GE	− .39	− .98	.04	.34	.87	− .78	− .35
	GG	.69	.47	− .45	.43	−1.42	− .28	− .16

* Includes only Sittings 2–6.

TABLE IIa

SUMMARY OF THE NUMBER OF POSITIVE AND NEGATIVE VALUES OF DIFF./σ FREQ. FROM SEQUENCES NOT SEPARATED BY INTERVENING JUDGMENTS

Seq.	Subjects											
	Ca		*Is*		*M*		*Sl*		*R*		All	
	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.
LL	0	5	0	4	1	5	3	3	1	4	5	21
LE	2	3	2	2	1	2	1	5	4	1	10	13
LG	5	0	4	0	4	2	5	1	3	2	21	5
EL	4	1	2	2	1	2	5	1	5	0	17	6
EE	0	5	0	4	1	2	0	6	0	6	1	23
EG	5	0	3	1	3	0	6	0	6	0	23	1
GL	2	3	3	1	5	1	2	4	1	4	13	13
GE	5	0	4	0	2	1	6	0	6	0	23	1
GG	0	5	0	4	1	5	0	6	0	6	1	26
Rep.	0	15	0	12	3	12	3	15	1	16	7	70
Total	23	22	18	18	19	20	28	26	26	23	114	109

Table Ia gives the value of Diff./σ freq. for each of the nine possible sequences of judgments for each *S*, fractionated by sittings of 324 judgments and covering sequences of judgments whose members are not separated by intervening judgments. In the first column, under the caption of "Seq.," appear the nine possible judgment sequences, beginning with judgments of *lighter* followed by judgments of *lighter*, and ending with judgments of *greater* followed by judgments of *greater*. The initial "L" stand for *lighter*, "E" for *equal*, and "G" for *greater*. In the second column appear the values for each of the Diff./σ freq. computed from the data

TABLE IIb

SAME AS TABLE IIa EXCEPT THAT SEQUENCES ARE SEPARATED BY A SINGLE INTERVENING JUDGMENT

Seq.	Subjects											
	Ca		*Is*		*M*		*Sl*		*R*		All	
	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.
LL	0	5	0	4	1	5	3	3	2	3	6	20
LE	5	0	4	0	2	1	2	4	3	2	16	7
LG	3	2	4	0	3	3	5	1	4	1	19	7
EL	4	1	2	2	2	1	6	0	4	1	18	5
EE	0	5	0	4	1	2	3	3	1	5	5	19
EG	5	0	4	0	2	1	3	3	4	2	18	6
GL	3	2	3	1	4	2	2	4	2	3	14	12
GE	4	1	1	3	1	2	3	3	5	1	14	10
GG	1	4	1	3	2	4	3	3	1	5	8	19
Rep.	1	14	1	11	4	11	9	9	4	13	19	58
Total	25	20	19	17	18	21	30	24	26	23	118	105

TABLE IIc

SAME AS TABLE IIa EXCEPT THAT SEQUENCES ARE SEPARATED BY TWO INTERVENING JUDGMENTS

Seq.	Subjects											
	Ca		*Is*		*M*		*Sl*		*R*		All	
	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.
LL	2	3	2	2	2	4	4	2	3	2	13	13
LE	4	1	2	2	1	2	3	3	5	0	15	8
LG	2	3	1	3	3	3	2	4	2	3	10	16
EL	3	2	2	2	1	2	1	5	4	1	11	12
EE	1	4	1	3	1	2	4	2	5	1	12	12
EG	5	0	2	2	1	2	3	3	1	5	12	12
GL	2	3	2	2	5	1	5	1	1	4	15	11
GE	3	2	2	2	2	1	2	4	1	5	10	14
GG	2	3	4	0	1	5	2	4	5	1	14	13
Rep.	5	10	7	5	4	11	10	8	13	4	39	38
Total	24	21	18	18	17	22	26	28	27	22	112	111

taken during the first temporal sitting. In the succeeding columns are found similar values computed from data taken during successive sittings. In the final column all the data have been consolidated into a single distribution to give a single value of Diff./σ freq. for all sequences of judgment on equal weights in which the members of the sequences have not been separated by intervening judgments. All negative values are in bold-faced type.

Tables Ib, Ic and Id are identical in form with Table Ia. The data contained in Table Ib have been computed for sequences whose members are separated by one intervening judgment while the data in Table Ic have been computed for sequences whose members are separated by two inter-

TABLE IId

SAME AS TABLE IIa EXCEPT THAT SEQUENCES ARE SEPARATED BY THREE INTERVENING JUDGMENTS

Seq.	Subjects											
	Ca		*Is*		*M*		*Sl*		*R*		All	
	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.	Pos.	Neg.
LL	2	3	1	3	1	5	3	3	3	2	10	16
LE	3	2	2	2	1	2	2	4	2	3	10	13
LG	4	1	2	2	4	2	3	3	3	2	16	10
EL	3	2	3	1	3	0	2	4	2	3	13	10
EE	2	3	1	3	1	2	2	4	2	4	8	16
EG	5	0	2	2	1	2	4	2	4	2	16	8
GL	3	2	3	1	5	1	3	3	3	2	17	9
GE	3	2	4	0	2	1	3	3	3	3	15	9
GG	0	5	1	3	1	5	3	3	3	3	8	19
Rep.	4	11	3	9	3	12	8	10	8	9	26	51
Total	25	20	19	17	19	20	25	29	25	24	113	110

vening judgments and the data in Table Id for sequences whose members are separated by three intervening judgments.

Tables II a–d give a summary of the consistency with which positive and negative values of Diff./σ freq. are encountered in Tables I a–d. In the first column of Table IIa appears a list of the various judgment sequences. Following it are columns devoted to the various *S*s. Each *S*'s column is divided into two sub-columns, in the first of which appears the number of postive differences, and in the second the number of negative differences found. Tables IIa, IIb, IIc, and IId summarize Tables Ia, Ib, Ic and Id respectively.

Results for M. The results for *M* are difficult of interpretation because of a shift in his attitude towards the giving of *equal* judgments, a shift which began with the second sitting and was not completed until the third. In the first sitting *M* gave a very large number of *equal* judgments. In the second and third he gave very few and in the fourth, fifth, and sixth he gave none. Because of this fact the results of the first sitting were not used in the Diff./σ freq. computed for consolidated distributions. Because his shift in attitude towards the *equal* judgments seriously affected the frequency of the *equal* category only, reference to the results which relate to the category *equal* for the most part has been omitted from the summary in the case of this *S*. However in respect to his repetition of *lighter* and *greater* judgments, we have included his results in the summary. It will be seen from the tables and their summary that nothing contained therein contradicts the more striking evidence of the other *S*s.

Summary of Results

(1) Perfect consistency of negative values indicating the avoidance of the immediate repetition of the judgments *equal* and *greater* is shown by all *S*s in all sittings except with *M*, whose attitude shifted in the early part of the experiment. Two *S*s give perfect consistency of negative values in the immediate repetition of *lighter*. The consolidated data show the negative difference values on repeated judgments to be statistically significant in 5 instances, above 2.00 in 7 instances, and above 1.00 in 11 cases out of 15. All 15 are negative in sign.

(2) Consistency in the negative values in the case of repeated judgments continues when the sequences are counted with one intervening judgment. *Ca* and *Is* show only 1 positive value in this case in all sittings; *M* and *R*, show only 4 positive values; and in the case of *Sl*, the positive and negative values are equally divided in number, 9 apiece. The consolidated distributions show fewer statistically reliable values. One is above 3.00, 2 are above 2.00, and 6 are above 1.00. Twelve out of 15 of the difference values for repetition of a judgment category are negative.

(3) The consistency of appearance of negative values through the vari-

ous sittings in the case of sequences with two intervening judgments continues with *Ca* and *M*, but is not present in the case of the others. Statistically reliable values computed on the consolidated distributions are absent and no consistency shows in the number of negative values among the combined results of the subjects.

(4) The consistency of the appearance of negative values through the successive sittings in the case of sequences with three intervening judgments is present with *Ca*, *Is*, and *M*, but is not present in the case of the remaining *S*s. Statistically reliable values are not seen in the consolidated data but 10 out of 15 of the difference values are negative.

(5) The judgment categories are not equally affected by the avoidance of repetition. The magnitude of the values Diff./σ freq. are consistently greater in the case of the *equal* category in the cases of all *S*s but *M* and *R*. The category *lighter* is unquestionably least affected by the avoidance of repetition.

(6) Evidence is at hand indicating that the preferred judgment immediately following a judgment of *lighter* is *greater*, an observation which confirms Arons and Irwin's results and which is demonstrated here both by the consistency of its appearance through the sittings, and by the consistency among the results of all the *S*s. The evidence for a consistently preferred judgment after *equal* points to the *greater* category. After judging equal weights *greater*, the preferred judgment is definitely equal. This fact is indicated by the consistency of its appearance within the sittings, the magnitude and direction of the difference values computed in the consolidated series, and the consistency of positive difference values in the case of the *greater-equal* sequence computed in consolidated distributions.

(7) Whatever preferences are shown in the category of judgments immediately succeeding a given judgment have practically disappeared when the sequences with one intervening judgment are examined. For the remaining types of sequences no preferences are to be found.

Discussion

Three results of this experiment are particularly important to the theory of the shifting of the effective value of a standard weight towards the effective value of the preceding weight. They are:

(1) The fact that large and reliable negative values of Diff./σ freq. are found in the case of the repetition of equals.

(2) The fact that after a judgment of *greater*, a definite tendency exists for the report *equal* on the succeeding judgment.

(3) The fact that contrast effects between pairs which are separated by intervening weights exist.

With respect to (1), the theory demands that no shift of the standard weight take place. After a judgment of *equal* the succeeding standard weight is fixed as neutral by the theory. Granted that it is so fixed, the second judgment still appears to be affected by another factor which predisposes to a change in the category, for the disposition against the repetition of *equal* is the most firmly established of all the results of this study.

With respect to (2), the theory demands that after a judgment of *greater,* the succeeding standard weight shift towards the greater end of the continuum. Turner publishes results showing a change in the threshold to accord with this expectation.[7] With the thresholds, computed by linear interpolation, he gives values of probable error in which Gauss' h appears as a parameter. He does not give the measures of precision separately. From the evidence presented here, it would appear that the effects of previous comparison in the case of judgments of *greater* would show in values other than the limens for *lighter* and *greater,* in an increase in precision and in the size of the interval of uncertainty, for example. Such a fact would imply that previous comparisons do not affect the measure of sensitivity alone.

With respect to (3), an interesting situation ensues. To apply the theory of the shifting of effective value of the standard weight to this fact it is necessary to assume that the perception of every standard weight is affected by memory traces arising from the perceptions of the last two or three pairs and that these traces can be carried over the experience of lifting and judging interposed pairs of weights. No evidence, apart from that contained in this study, dealing with the length of time through which such an effect endures, is at hand. On the basis of the third fact, however, the assumption of the endurance of memory traces as an explanation for the deviation of the found from the expected number of judgment sequences is not warranted. It is less of an assumption to postulate an attitude against the giving of repeated judgments. This is particularly true because the effects which best resist the passage of time and the lifting of interposed weights are those which deal with the repetition of the judgment *equal.* Therefore it cannot be concluded that *all* contrast effects are due to stimulus-conditions. Whatever may be true of the effects of stimulus on the judgments in psychophysical experimentation, it is also true that the *S*'s attitude is a constant and important factor in the formation of any given judgment.

[7] Turner, *op. cit.,* 8-9.

Conclusions

(1) Subjects in psychophysical experimentation tend not immediately to repeat the categories of judgment.

(2) After the judgment *greater,* the judgment *equal* is preferred.

(3) The avoidance of repetition is most marked in the category *equal.*

(4) These effects are noticeable, not only in the judgments immediately following a given judgment, but also in the judgments for pairs one or more removed from the first.

(5) Contrast effects cannot be explained completely by stimulus conditions.

(6) The disposition against the repetition of a judgment is best explained at present as due to an attitude by the *S.*

THE EFFECT OF AUTOMOBILE DRIVING ON THE REACTIONS OF THE DRIVER

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While there is abundant evidence from every day life that fatigue of automobile drivers, particularly that associated with dozing, causes accidents,[1] there is very little experimental evidence bearing upon the effect of an ordinary day's drive on the operator's efficiency and proneness to accidents.

The purpose of this research is specifically to determine the effect of a moderately long day of driving on the efficiency of the driver, and thus indirectly to throw some light on the causes of accidents.

The data were obtained (1) from laboratory tests made before and after drives, (2) from records obtained automatically at the steering wheel during the course of the drives, and (3) from the accidents of our drivers in the course of the study. The present paper presents only the results of the laboratory tests. A second paper will deal with the steering records and the accidents.

A series of tests was chosen which required in their performance reactions resembling in principle those required in automobile driving. The tests were sufficiently simple to permit of accurate scoring of the efficiency of the drivers in their performance.

Driving requires, among other things, (1) fine discriminations through the visual and kinesthetic senses, enabling quick and accurate muscular responses in the operations of steering, braking and accelerating the car; (2) sustained attention; and (3) complex motor responses. These considerations led to the choice of the following tests: the color naming and mental addition tests which require sustained attention, speed and accuracy in the performance of composite pattern reactions; the postural steadiness test which depends in a large measure on the acuteness of deep sensibility; the hand-eye coördination test, requiring for its performance fine discriminations of visual and deep sensibility with coördinated movement; and a test of visual efficiency. To the above tests of performance was added one entirely of a non-performance character, the vascular skin-reaction.

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[1] J. S. Baker, O. M. Gunderson and S. J. Williams. Too long at the wheel. *Pub. Safety Pamphlet No. 33. Natl. Safety Council, Inc., Chicago,* 1935, 1-48.

METHOD AND PROCEDURE

Tests:(1) Vascular skin-reaction. The vascular skin-reaction is obtained by making a stroke on the skin with a blunt instrument. After making the stroke, a period of several seconds elapses before any change is noted in the skin. A faint white streak then appears which increases in intensity until the stage of maximum whiteness occurs. It then begins to fade. The interval between the making of the stroke and the beginning of fading of the streak is timed with a stop watch and recorded as the fading time.[2]

Great differences occur between individuals in the intensity of the streak obtained after stroking the skin. In order to obtain accurate records of the fading time of the streak, only those subjects may be used who react with an intense white streak. Considerable practice is also desirable in making the observations.

The stop watch used in recording the fading time of the streak is started simultaneously with making the second of two strokes which are made, one immediately after the other on the forearm about 1¼ in. apart. The watch is started with the dial out of sight and in such a position that the ticking cannot be heard. When the fading of the streak is first perceptible, the watch is stopped. Every reading should be recorded. Three trials are made, one on the lower portion of the forearm, one in the middle, and one on the upper portion, and the average of the three readings is obtained as the fading time for the test. The fading time is generally longer in the lower part of the forearm than in the upper part.

(2) Postural steadiness. The amount of body sway while standing with the arms folded across the chest and the eyes closed was measured with an instrument devised by Miles, and called by him an ataxiameter.[3] As Miles has pointed out, the degree of steadiness maintained in this posture depends in a large measure upon the acuteness of the deep sensations from the muscles and joints. He has indicated, however, that it is affected also by cutaneous sensations from the feet. Our *S*s, therefore, wore the same shoes throughout the tests. Coats were removed before the test was begun.

S was allowed to stand in this apparatus for 20 sec. so as to permit him to become settled and adjusted to it before his record was taken. The body sway was then measured for a period of 1 min. The same process was repeated until 3 separate records of one minute each were obtained. An average of the 3 records constituted the *S*'s score. Three records were taken so that the degree of variability between the separate tests could be computed.

(3) Hand-eye coördination: Form A. The instrument used in the performance of the hand-eye coördination tests was a modification of one employed by Swope,[4] which was in turn an improvement of a standard piece of laboratory equipment.[5] Swope's instrument, however, was used in one series of tests.

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[2] Ryan and Gordon have designed an instrument to exert uniform pressure in making the stroke. They found the fading time of the streak to decrease after short periods of intense muscular exertion, such as hill climbing; the regular day of work of hospital nurses; and loss of sleep. (Cf. A. H. Ryan and J. H. Gordon, The quantitative measurement of general fatigue. *Amer. J. Physiol.*, 45, 1917, 537-538.)

[3] W. R. Miles, Static equilibrium as a useful test of motor control, *J. Indus. Hygiene*, 3, 1922, 316-331.

[4] A. Swope, The steadiness meter, *Engin. Bull., Purdue Univ.*, 17, (no. 3), 1933, 115-127.

[5] G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 156 f.

The instrument was modified by us in two ways. First, the visual sensory factor in the test was accentuated. This was accomplished by using a ground glass plate within the box as a stop against which the stylus was inserted, and illuminating the ground glass plate from within. The depth of insertion was reduced to ½ in. When the stylus is inserted an inch without illumination, as with Swope's instrument, it is impossible to discriminate visually small differences of distance between the stylus and the edge of the hole. The test under these conditions, therefore, would appear to measure in a relatively greater degree the accuracy of kinesthetic discriminations. Since we had another test for the deep sensibility in which the visual element was eliminated (the postural steadiness test), we wished to devise a test which, in combining the use of the visual and kinesthetic senses, would do so in a manner that would permit of the best possible visual discriminations throughout the reaction.

The second modification of the instrument consisted of changing the relationship of the size of the holes in a way to permit of more accurate and convenient scoring of the results. This was accomplished by making the smallest hole ⅛ in. in diam., and increasing the diameter of each succeeding hole by 10% of the diameter of the whole next smaller in size. The diameter of the stylus was 3/32 in.

This arrangement permitted of readily computing changes in the score of every *S* after driving in terms of percentage change of accuracy of movement regardless of his relative skill in the test. A change, thus, in the average score of an *S* from hole 18 to hole 17 would be of similar magnitude as a change from hole 15 to 14, a relationship that did not exist in the previously devised instruments.

The brass plate of the instrument had 20 holes arranged in two rows 1½ in. apart, the smallest hole being at the extreme left end of the lower row. The brass plate was mounted on the base with the plane of its surface approximately at right angles to the axis of the stylus as it was ordinarily held by a subject in performing the test.

The instrument was placed on a table and *S* sat in a comfortable position before it. A metronome beating 60 to the minute ran throughout the test. At a signal from the experimenter, *S* began the test by inserting the stylus first into the largest hole, and then into each succeeding hole, in time with the metronome, until he made a contact with the edge of a hole. On making a contact, he rested his hand on the table for a count of 5 beats of the metronome, and repeated the process. Five trials were made and recorded, the average being the score of the test.

Form B. The second form of the hand-eye coördination test differed from the first only in that it measured *S*'s efficiency in a long series of insertions of the stylus into a single hole. In the first form, *S* might, through spurts, theoretically conceal some latent impairment. The first form was possibly not severe enough to disclose the degree of efficiency of hand-eye coördination required in driving, since driving requires for very long periods, continuous coördinated reactions to visual sensations.

The test consisted of having *S* insert the stylus 180 times, at the rate of 1 per sec., into a hole slightly larger than the hole determined by his average score in Form A.

A record was kept of the number of insertions in which contacts were made, for each minute of the test, and his score was computed as the percentage number of contacts made in the 3-min. test. A separate set of holes at one end of the metal plate was used for this test to avoid the possibility of one test interfering with the other.

(4) Visual efficiency. The test of visual efficiency used in our experiments was essentially the same as that employed by Ferree to study the effects of reading under

various kinds of illumination.[6] The apparatus consists of a bar about 2 m. long on which is mounted a scale graduated in millimeters. A wooden block bearing the test image rests on this scale and may be moved to a measured point on the scale determined for every *S*. The test object consisted of the letters "li" drawn with India ink on a white card. The letters were two millimeters long and spaced one millimeter apart.

S's head was held in a fixed position by means of a biting-board and the tests were made in a dark room with a source of illumination that was constant throughout the tests.

Prior to the administration of the test the *S*s spent approximately 55 min. in the laboratory with drawn shades, and of this time a period of approximately 15 min. was spent in the dark room.

The test was begun by seating *S* comfortably at the end of the scale, adjusting the test object at the proper distance on the scale, and placing a stop watch in *S*'s hand. At a signal from *E*, *S* took the bit in his teeth and closed his eyes for a period of 10 sec. At a second signal he opened his eyes and fixed the test letters with his eyes for a period of 3 min., timed by *E*. *S* was free during the test to blink as often as desired.

During this period the test object became alternately clear and blurred. Every time the object became blurred *S* pressed the stem of the stop watch, and pressed it again when it cleared. *E* recorded the number of seconds elapsing between the beginning of the test and each blur, the total number of blurs, and the total time of blurring registered on the stop watch by the subject.

(5) Color-naming. The color-naming test employed was a further modification of the one devised by Cattell and Farrand[7] and modified by Lee and Kleitman in their studies on the effect of loss of sleep.[8] The materials required for the performance of this test consist of sets of 3 white cards 10 in. square, on each of which is mounted 100 colored squares (1 cm. sq.) arranged in 10 rows of 10 each. Each card contained approximately the same number of each of the 10 colors arranged in chance manner. A stop watch was used to time the reading of the card and a screen was placed between *E* and *S* to avoid distraction.

The colors were read in a darkened room under a 300-watt Mazda ceiling light in order to obtain constant conditions of illumination. After reading the colors on each test card, it was given a quarter turn which resulted in a different sequence of colors. The reading of the colors was continued until 400 colors were named for each card, or 1200 from the 3 cards used for each test.

A typewritten key was prepared for the color sequence of each card in each position, and this key was used by *E* in observing the errors made. This method of determining the errors seemed better than the use of duplicate cards, because it reduced *E*'s tendency to error.

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[6] C. E. Ferree, Tests for the efficiency of the eye under different systems of illumination and a preliminary study of the causes of discomfort, *Trans. Illum. Engin. Soc.*, 8, 1913, 40-57.

[7] J. M. Cattell and L. Farrand, Physical and mental measurements of the students of Columbia University, *Psychol. Rev.*, 3, 1896, 618-648.

[8] M. A. M. Lee and N. Kleitman, Studies on the physiology of sleep: Attempts to demonstrate functional changes in the nervous system during experimental insomnia, *Amer. J. Physiol.*, 67, 1923, 141-151.

S was instructed to name the colors as rapidly as possible, and to correct any errors which he observed.

A separate record was kept of the corrected and uncorrected errors made on each card, or on every 400 colors named, and of the time required to name them. A more thorough analysis was thus possible of the results of this test than has been previously made.

By having 6 cards, it was possible to change the combination of cards so that a different set was used morning and evening.

(6) Mental addition. The mental addition test employed was, in general, similar to the one used by Weiskotten in his study of the effect of loss of sleep on the speed of mental processes.[9]

The material used in the test consisted of cards having seven typewritten columns of 15 digits each. Three cards were used at each test. The digits were arranged in a different order in each column of each card, but each column contained approximately the same weighting of the numerals used, so that the answers ranged between 70 and 100. A key was made of the correct answers for each column.

By means of a stop watch, a record was made of the time required to obtain correct answers to all of the columns of each card. If *S* made an error on any column, he was required to repeat the addition until the correct answer was obtained. The cards were placed on the table in front of *S*, but he was not permitted to follow the figures with his finger while making the additions. A small screen placed between *S* and *E* prevented distraction of *S* by the notations made by *E*.

S's score was the time required for him to add correctly all of the columns on 3 cards. By keeping a separate record of errors and time on each card, it was possible, however, to obtain a measure of the speed of addition apart from the slowing due to errors, and the variability of efficiency between the 3 cards.

The standard drive. In order to obtain results that might have the possibility of becoming quantitatively significant with reference to the fatigue effect of driving, it was decided to obtain a relatively large number of observations under standardized conditions of driving time and distance. A distance was decided upon that was fairly long, but no longer than the distance driven by commercial drivers during the course of an average day. The conditions of the driving day were kept as uniform as possible throughout the tests.

The drivers were scheduled so that each man drove on alternate days. Six drivers were engaged, driving in groups of 3 and using 3 cars. No passengers were carried.[10]

The drivers arose at about 6:30 A.M., reported regularly at the garage at 8:00 A.M., and drove until 9:00 A.M. over a definite highway through the city, covering a distance of approximately twenty miles. At 9:00 A.M., the drivers reported at the laboratory where the tests were given, always in the same sequence of tests and drivers. The tests were completed in approximately an hour and a quarter. After obtaining gasoline and oil, and checking the pressure of the tires, the remainder of the drive was begun at approximately 10:30 A.M.

There was a stop of about an hour for a hearty luncheon around 1:00 P.M., and a

[9] T. F. Weiskotten, On the effects of the loss of sleep, *J. Exper. Psychol.*, 8, 1925, 363-380.

[10] In an earlier series of test drives made in 1934, four drivers were engaged, using two cars.

stop for a plain malted milk drink at approximately an hour before their return in the evening. They returned to the garage at approximately 7:30 P.M., after covering a total distance of about 300 miles. There was a walk of 6 min. to the laboratory from the garage, after which the evening tests were begun. The actual driving time varied between 8½–8¾ hrs. On the road the men alternated in the positions of leading and following for approximately equal distances.

The drive of one hour's duration before the morning tests was made for the purpose of standardization. We wished both test-periods to be preceded by similar activities so that the differences in the *Ss'* performances in the tests at the two periods would be due to amount of previous activity and not to kind.

Two different routes were followed by the drivers. The routes were carefully planned and standardized so as to minimize the possible effects of conditions other than the driving itself.

During the cold weather the automobiles were heated with hot water heaters and ventilated through the right rear window. The automobiles were all new sedans, representing five of the most popular makes.

Control days. The laboratory tests were made on the drivers on control days—days in which light activity or recreation was substituted for the long drive—in the morning and evening as before. All the conditions of the tests on the control days duplicated as nearly as possible those on the driving days. The light activity consisted of taking the automobiles to the garage for servicing or of doing other errands in connection with the upkeep of the automobiles. Recreation consisted of either playing checkers or cards, visiting a gymnasium and observing boxers train, attending a motion picture performance, or spending the day at home lounging about.

The tests were given at approximately the same hours and in the same sequence. The mid-day meal and afternoon drink were taken at approximately the same hour as on driving days.

Both the morning and evening laboratory tests on the control days were preceded by approximately 1 hr. of driving similar to the preliminary drive made on the regular driving day.

The control days thus differed from the driving days in having light activity or recreation substituted for approximately 6¾ hours of driving, the amount by which the driving days exceeded that of the control days.

Selection and control of drivers. The drivers ranged in age from 19-30 yrs. All were high school graduates, and two were college graduates. Three were employed part time on alternate days. All lived at home in families with good home conditions. Daily histories were kept of the amount of sleep each night and of the activities of the off days.

The drivers began the test work with the understanding that they were to get a good sleep on the night before the tests. The histories showed that they did. Generally, they slept late the morning after the drive.

The drivers were told that the tests were made in connection with studies of driving ability. This explanation resulted in great effort on their part to make good scores after the drives. They were told also that any further discussion of the tests either with us or amongst themselves would interfere with our purpose. They seemed readily to understand this, and their attention seemed continually directed toward trying to improve their test scores, both morning and evening.

Their attention was further diverted from the object of the tests by having careful measurement made and records kept of gasoline and oil consumption of the cars. The keeping of these records was made partly to impress them with the seriousness, care and accuracy of the work.

The instructions and procedure were designed to avoid any intimation that there was any excuse for making poorer test scores in the evening than in the morning.

The majority of the drivers were from families in "moderate circumstances," jobs were scarce, and they all needed the pay they received. Because of careful selection of drivers, the instructions given them, and the relationship maintained with them in the laboratory, it seemed that their best effort was made at all times in the performance of the tests. On one test, hand-eye coördination: form A, the drivers would know their best scores at each test, and in a general way, their average scores. In none of the other tests did they ever know how well they performed them.

TABLE I

SUMMARY OF RESULTS OBTAINED ON 128 DRIVING DAYS AND 28 CONTROL DAYS

(Showing the mean percentage change in the evening scores as compared with the morning scores)

Tests	Driving days			Control days		
	M	P.E.	$P.E._M$	M	P.E.	$P.E._M$
Vascular skin-reaction	−12.38	3.51	0.35	− 1.21	3.78	0.79
Postural steadiness	+ 8.32	11.31	1.03	+ 0.20	9.80	1.85
Hand-eye coördination: Form A	− 2.92	6.50	0.59	+ 0.39	3.66	0.69
Hand-eye coördination: Form B	+23.58	29.38	2.68	− 9.78	17.78	3.36
Visual efficiency	− 2.62	1.68	0.17	− 0.80	1.05	0.22
Color naming: Time	+ 3.93	4.01	0.37	− 1.65	2.68	0.51
Color naming: No. Errors	+46.79	43.80	4.00	−16.28	16.47	3.11
Mental addition	+ 8.46	10.16	0.93	− 3.59	5.87	1.11

Before beginning the driving, the *S*s came to the laboratory for three days for instruction and practice in the tests. In spite of these preliminary drills, considerable improvement occurred during the driving period in the performance tests through learning. This did not interfere with the use of the test scores to show the effect of a day of driving because this effect was recorded as the percentage change in the evening score as compared with the score of the same morning.

Sublingual temperature was obtained before and after the drives on a considerable number of days. This was obtained with a 1-min. clinical thermometer placed under the tongue for a period of 5 min. before reading. The temperature record was not obtained until after *S* had been in the laboratory for at least half an hour without having had anything introduced into his mouth.

RESULTS

The test score obtained after the standard drive for each test for every *S* was divided by the test score determined before the drive, and from this computation was obtained the percentage change in the test score after the drive. Table I summarizes the results of 120 drives and 28 control days on 6 drivers.

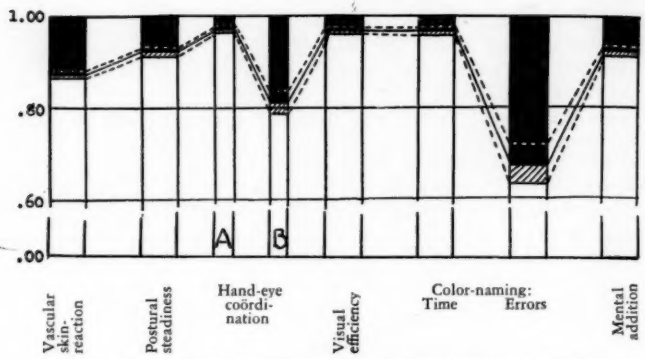

FIG. 1. CHANGES OCCURRING IN THE TESTS AFTER THE DRIVES

(Based on 120 driving days with 6 *Ss.*)

The solid and shaded areas indicate the average loss after the drives. Scores before the drives are represented as 1.00. The dotted lines show the extent of the variability of the $P.E._M$

Figs. 1 and 2 present graphically the average results for all tests made on driving and control days. In the graphs, the averages for the morning scores of all tests are indicated as 1.00. The solid black areas indicate the average amount by which the evening scores deteriorate from the morning level, and the areas shaded with vertical lines indicate the average amount

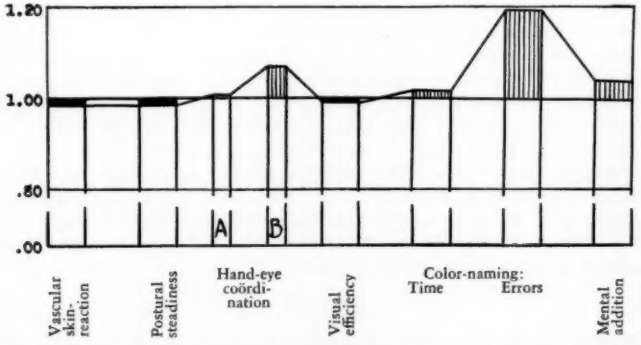

FIG. 2. CHANGES OCCURRING IN THE TESTS ON THE CONTROL DAYS

(Based on 28 days with 6 *Ss.*)

The solid areas indicate the average loss and the vertically shaded areas the average improvement in the evening scores. The morning scores are represented as 1.00.

by which the evening scores improve over those of the morning. The dotted lines running from each vertical bar to the next indicate the magnitude of the probable error of the mean for each test, while the solid line between the two dotted lines joins the means for the different tests.

Since the average test scores for several of the tests were based on 3–5 trials or parts, it was possible to determine the degree of variability between the separate trials of the tests before and after the drives. The measure of

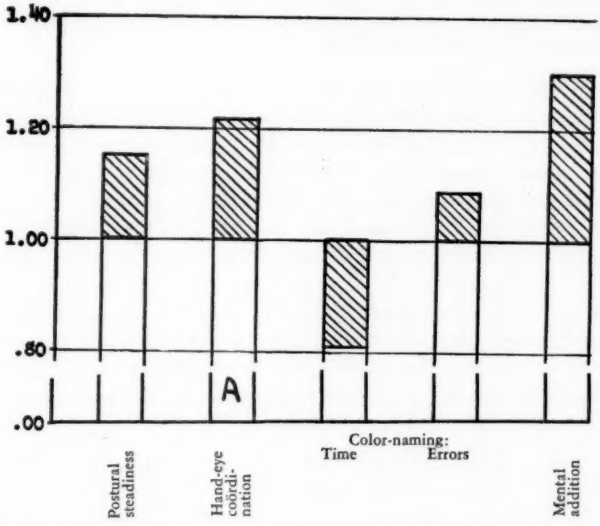

FIG. 3. COMPARISON OF THE VARIABILITY OF PERFORMANCE BEFORE AND AFTER THE DRIVES

Shaded areas indicate the change in variability after the drives compared with the variability before the drives, which is indicated as 1.00.

variability used was the standard deviation of the separate trials of each test from the mean.

Fig. 3 shows the average variability of the test trials made before and after the drive. The average variability within the tests made before the drive in the case of all tests is indicated in the graph as 1.00. The diagonally shaded portion indicates the average variability within the evening test scores as compared with the morning variability.

The effect of the standard automobile drive on the performance or score of each test will now be described.

(1) Vascular skin-reaction. A decrease in the fading time was observed after the drives in the case of every driver for every driving day. The average decrease in time was 12.4%. Of the 23 control days, a decrease in the fading time was observed on 12 days; an increase on 10 days; and no change on 1 day. (The record of results obtained on one driver was excluded because his streak was not sufficiently intense to permit of accurate observations.) The average decrease for all of the control days was 1.2%. The difference between the mean percentage change in fading time on the driving days and the mean for the control days is 12.93 times the probable error of the difference between the two means.[11]

(2) Postural steadiness. On 78 out of 120 driving days there was an increase of body sway after the drive over the amount occurring before the drive; in 40 cases there was a decrease; and in 2 instances there was no change. There was an average increase in the amount of body sway after the drive for all driving days of 8.3%.

On the 28 control days, there was an increase of the body sway in the evening over the amount occurring in the morning on 13 days; while on 15 days there was a decrease. For all control days there was an average increase of 0.2%. The difference between the mean change observed on driving days and the mean for control days is 3.83 times the probable error of the difference between the two means. It may be seen in Fig. 3 that the variability within the test scores after the drives is considerably greater than in the scores made before the drives.

(3) Hand-eye coördination: Form A. When the morning and evening scores made in Form A of the hand-eye coördination test were compared it was found that the evening scores were poorer in 65 cases, better in 42, and unchanged in 13. The average of all the scores, however, showed a decrease in the accuracy in the evening tests of 2.9%.

On the control days, 9 instances occurred in which the evening score was poorer than the morning, 13 cases in which it was better, and 6 cases in which there was no change. For all the control days there was an average improvement of about 0.4%.

While these results suggest a decrease in the accuracy of hand-eye coördination after the standard drives, the difference between the average morning to evening change in the scores on driving days and control days is only 3.64 times the probable error of the difference between the averages.

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[11] The probable error of the difference was obtained by the following formula:

$$P.E._D = [(P.E._1^2/n_1) + (P.E._2^2/n_2)]^{1/2}$$

It may be seen from Fig. 3 that the variability within the test scores made after the drives is more than 20% greater than the variability within the scores made before the drives. This observation suggests that the scores, based as they are on an average of 5 test trials each, do not entirely disclose the comparative accuracy of the hand-eye coördination of the drivers before and after the drives.

To throw more light on this point, a tabulation was made of the best and the poorest scores made by every driver in the 5 trials in all the morning and evening tests. It was found from this analysis that the average of the poorest scores made after the drives were 6.7% less accurate than the average of the poorest scores made before the drives. This decrease is more than twice as great as that shown when the averages of all the trials are compared. Furthermore, the average of the best scores made after the drives were 2.4% less accurate than the average made before the drives.

Form B. Form B of the hand-eye coördination test is longer than Form A. Greater neuro-muscular demands were, therefore, made upon the *S*s by Form B.

When the scores made in this test after the drives are compared with those made before the drives, they are poorer on 78 days and better on 34 days. There was an average increase of 23.6% in the number of errors (or contacts) made after the drives over the number made before the drives.

On the control days there were 17 instances in which the evening scores were better than the morning scores; 9 in which the scores were worse; and 2 in which there were no change. For all control days there was an average improvement of the evening score over the morning score of 9.8%.

The difference between the average percentage change on the driving days and the average change for the control days is 7.76 times the probable error of the difference between the two averages.

In order to throw more light on the manner of performance of this test, a tabulation was made of the error score for each of the 3 succeeding minutes of each test. From this tabulation were obtained averages for all drivers on each third of the test, both before and after driving. The results are shown in Fig. 4, from which it is seen that there is a decrease in the number of errors made in the second third of the test both before and after the drive, while the average number of errors is greatest in the last third of the test. Furthermore, the average number of errors made after the drive for each of the three parts of the test is greater than for any part of the test made before the drive.

The improvement in the second third of the test is probably a practice effect seen so frequently in manual activities and in industrial operations. The rise occurring in the errors in the final third of the test is probably due to the neuro-muscular strain of the test itself. The similarity of the shape of the curves of performance before and after the drives is evidence of the uniformity of test conditions, and of the attitude of the *S*s at both times of the day.

(4) Visual efficiency. The percentage of clear vision was less after the drive than before it on 89 of 100 driving days; while on 11 days there was an improvement. There was an average decrease of 2.6% in clear vision after the drives as compared with the percentage of clear vision before the drives for all of the driving days. (The results on one driver were excluded on this test for reasons that will be discussed presently.) On 23 control days, the percentage of clear vision in the evening was less than in the morning in 17 instances, and greater in 6. For all control days the percentage of clear vision in the evening was only 0.8% less than in the morning. The

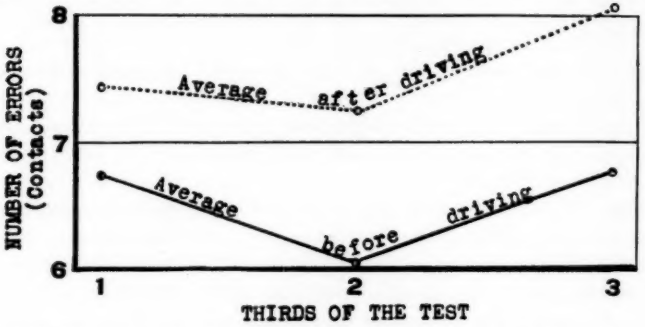

FIG. 4. COMPARISON OF THE PERFORMANCE IN FORM B OF THE HAND-EYE COÖRDINATION TEST BEFORE AND AFTER DRIVING

(Based on the averages of all the *Ss* in all tests.)

difference between the average loss of clear vision on driving days and on control days is 6.62 times the probable error of the difference between the two averages.

The results obtained with this test are perhaps more impressive if the comparison of visual efficiency before and after driving is expressed in terms of change in the percentage of blurred vision rather than clear vision. The average percentage of blurred vision for all driving days was 40.7% greater after the drives than before the drives, while the number of blurs increased by 40.4%. It appears from these figures that the increase of blurred vision, or the decrease of clear vision, that was observed to follow automobile driving was due chiefly to an average increase in the number of blurs which occurred rather than to an increase in the average duration of the blurs. The average number of blurs occurring before the drives was 3.16 for the 5 *Ss*.

The decrease in the percentage of clear vision after driving was not always, however, due to the greater number of blurs occurring after the drive. There are some cases in the test records in which there was a decrease in the percentage of clear vision with no increase in the number of blurs.

The records of one driver were omitted on this test because of the circumstance that several changes were made in the conditions of the test as it was administered to him. After the third day of driving, he showed 100% clear vision both before and after the drives when the test image employed for the other drivers was used. The test was then given before and after the drives on succeeding days with a prism over one eye, with the base in and out, and with both concave and convex lenses over one or both eyes.

This driver would quickly become accustomed to all of these devices and would perform the test without blurs. The images were then modified until finally one was found with which he reported blurring from 1 to 3 times during the test. This image consisted of a single row of very small vertical dots and spaces, 5 in number. With this image, *S* would report a fusion of 2 or 3 dots. This *S* was a boxer and boxing instructor whose work required a great amount of practice in visual alertness.

(5) Color-naming. The time required to perform the color-naming test was increased after the drives in 90 of the 120 observations; while it was decreased in 28 instances. There was an average increase of 3.9% in time required following the drives over the time required before the drives. On the control days there was an increase in the evening time required for the tests on 10 of the 28 days, while on the remaining days the evening time was decreased. For all of the control days there was an average decrease in the time required for the evening test of 1.6%. The difference between the means of the test days and the controls is 8.92 times the probable error of the two means.

There were 92 days on which the errors made in the performance of this test were increased; 20 days on which they were decreased; and 8 days on which there was no change. For all of the driving days there was an average increase of 46.8% in the errors made after the drives, compared with the errors made before the drives. On the control days the evening errors, compared with those of the morning, were increased on only 6 days; while they showed a decrease on 20 days. For all of the control days there was an average decrease of 16.3% in the number of errors made in the evening as compared with the number of errors made in the morning. The difference between the means on driving and control days is 12.45 times the probable error of the difference between the two means.

When the variability of the three parts of this test was compared morning and evening, it was found that the variability of time was greater in the morning, and the variability of errors greater in the evening. The increase in the variability of the morning over that of the evening time is contrary to the results obtained in all of the other tests analyzed. It suggests that the *S*s, because they are in a relatively fresh condition in the morning, are capable of attaining, through spurts, greater speeds in parts of the test than in the evening.

Fig. 5 shows the results of a further analysis of the data obtained in the three parts of this test, that is, in the time and errors made in naming every 400 of the

1200 colors. It is seen that there is an increase in the average time required, and in the average number of corrected errors, for each succeeding part of the test. It may be further noted that the time, corrected errors, and uncorrected errors begin at a higher level after the drive than before it.

The curve of uncorrected errors shows a marked increase in the middle 400 colors named over those in the first and third parts of the test. This is interesting in suggesting something of the manner of fluctuation of attention during the performance of a prolonged task in which speed is associated with elements that may be supposed to contribute to monotony—a combination of circumstances often occurring in driving.

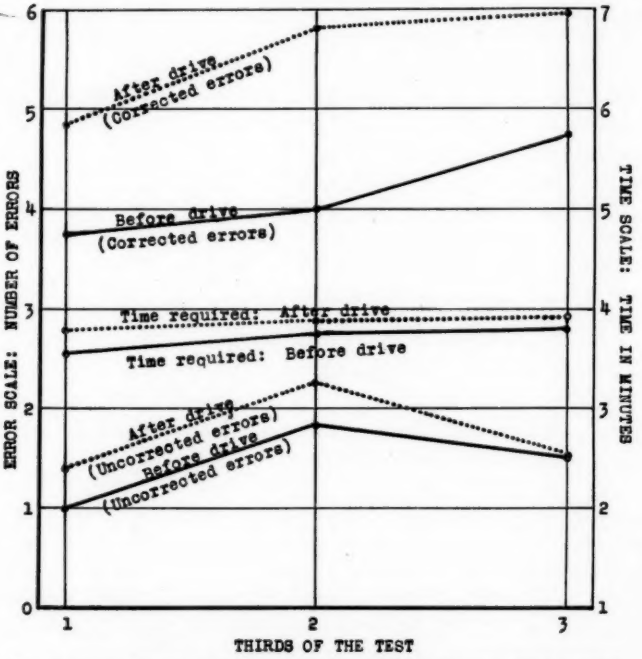
ERROR SCALE: NUMBER OF ERRORS
TIME SCALE: TIME IN MINUTES
THIRDS OF THE TEST
After drive (Corrected errors)
Before drive (Corrected errors)
Time required: After drive
Time required: Before drive
After drive (Uncorrected errors)
Before drive (Uncorrected errors)

FIG. 5. COMPARISON OF PERFORMANCE IN COLOR-NAMING TEST BEFORE AND AFTER DRIVING

(Based on the averages of all the *S*s in all tests.)

The progressive increase in the time and errors of each succeeding third part of both the morning and evening tests indicates the effect of the test itself on the ability to perform it. It also indicates that the test was performed under conditions of maximum effort.

(6) Mental addition. There was an increase in the time required for this test after the drives as compared with the time required before the drives

on 82 of 120 driving days and a decrease on 38 days. There was an average increase in time required for the test amounting to 8.5% for all driving days.

There were only 6 instances in the 28 control days in which the time required for this test was greater in the evening than in the morning, while

TABLE II

THE EFFECT OF AUTOMOBILE DRIVING ON MENTAL ADDITION

(Based on comparison of time required before and after drives for test problems having same number of errors.)

	S	Number of errors: 0 Av. time	0 No. items	1 Av. time	1 No. items	2 Av. time	2 No. items
Before Drive	H.V.	2.06	44	2.31	20		
	N.H.	1.95	42	2.28	21	2.62	9
	J.B.	1.45	47	1.74	20		
	H.M.	1.79	20			2.40	19
	K.M.	1.32	35	1.64	26	1.74	11
	W.S.	1.72	45	2.08	20		
	Total	10.29	233	10.05	107	6.76	39
	Av.	1.72		2.01		2.25	
After Drive	H.V.	2.28	40	2.40	27		
	N.H.	2.06	47	2.80	15	2.88	10
	J.B.	1.52	50	1.73	19		
	H.M.	1.86	16	2.14	19	2.32	25
	K.M.	1.43	29	1.71	28	1.96	9
	W.S.	1.81	39	2.01	22		
	Total	10.96	221	12.79	130	7.16	44
	Av.	1.83		2.13		2.39	
	%Change	+6.39		+5.97		+6.22	

on the remaining 22 days the time was decreased. For all control days there was an average decrease, or improvement, of 3.6%.

The difference between the mean percentage change in time on the driving days and control days is 8.31 times the probable error of the difference between the two means.

An average of 1.96 errors in additions was made on the driving days before the drives as against an average of 2.30 made after the drives, or an increase of 17.5%. This raises the question as to whether the increased time required in the performance of this test as a consequence of driving is due solely to the repetitions of additions resulting from the increased number of errors made, or whether there is a slowing of the mental processes involved in addition. The data contained in Table II make it quite clear

that there is an increase in the time required for the addition of a similar number of digits, amounting to an average of 6.4% for all drivers. This observation is based on a comparison of the average time required before and after the drives for the addition of cards in which no errors were made.

The effect of the drive is to produce, also, a greater variability of performance within this test than in any of the other tests, as indicated in Fig. 3. This is probably due to the relatively great effect of a single error on the score for each card, since an error required the repetition of an entire column which contains one-seventh of the numbers on the card.

(7) Sublingual temperature. Mouth temperature records were obtained for 47 man-days before and after the drives. On 27 man-days, the temperature was higher after the drive than before it; on 18 man-days there was a decrease in mouth temperature; and on 2 days it remained unchanged. When the averages of mouth temperature for each driver were determined, it was found that in the case of 4 drivers it was higher after the drives than before; in one case it was lower; and in one, unchanged.

The body temperature measurements were made in connection with the tests as a control procedure because of the inverse relationship found by Kleitman to exist between changes in body temperature and reaction-time, performance, and steadiness.[12] In the majority of our observations, the body temperature was higher after, than before the drives. The loss of efficiency observed to follow the standard automobile drive appears, then, to have occurred in spite of body temperature changes that might have been expected to have a tendency to exert the opposite effect.

Daily histories of drivers. The daily histories of activities and hours of sleep on the days and nights preceding the driving days and the control days were remarkably uniform.

Five drivers used coffee, all smoked, and none used alcoholic beverages. Those drivers who drank coffee or smoked tobacco were moderate users, and were told to standardize their habits so as to drink or smoke the same amount at about the same time on every day of the test, whether on driving or control days. Those drivers who drank coffee had one cup at breakfast, two hours before the morning tests began. No more coffee, tea or cocoa was drunk during the day. It was thought that this understanding would be more likely to insure complete coöperation and reliable histories than attempts to prevent the use of coffee or tobacco entirely.

Weather conditions. Although the series of drives summarized in Table I covered the period of February to May inclusive (1935), a previous series of drives giving very similar quantitative results was made in the period of May to August inclusive (1934), so that a wide range of weather conditions was encountered.

Discussion of Results

Moss has conducted experiments on automobile riders and drivers in studying the riding comfort of automobiles.[13] He devised an instrument which he calls a

[12] N. Kleitman, Studies on the physiology of sleep: Diurnal variation in performance, *Amer. J. Physiol.*, 104, 1933, 449-456.

[13] F. A. Moss, Further experimental work with the new wabblemeter, *Soc. Auto. Engin. J.*, 29, 1931, 243-246.

"wabblemeter" on which *S* balances himself while standing. A record is obtained from the platform of the shifts in pressure transmitted through the feet of the *S* to the instrument in maintaining his balance.

While he has reported an increase in "wabbling" in passengers and drivers after riding, which is especially pronounced over rough roads, it appears that the effect passes away quickly. The test given by him requires that the *S*s sit in the car until tested and step immediately from the automobile to the "wabblemeter" which is placed a few feet from the car.

Swope undertook experiments which might lead to finding some instrument that might be used to disclose the nervous or fatigue effects produced by riding in, or driving, an automobile.[14] Gripping tests, eyesight tests, and reaction-time tests were stated to yield inconsistent or non-significant results.

With the "steadiness-meter," however, he was able to obtain results which, as stated by him, measured with a fair degree of reliability some effects of driving and riding in an automobile. The instrument he used was similar to the instrument used in our hand-eye coördination tests, with the exceptions noted previously. His tests were given immediately after finishing the drive, either in the car or at a table placed a few feet from the car.

Swope found in certain series of tests a significant difference between the mean scores before and after driving, while in other series, the difference was not significant. He found that the variability of test performance at the end of the trip was greater than that at the beginning of the trip.

Moss and Swope gave their tests immediately after the completion of driving with instruments placed close to the stopping point of the cars. Although any deterioration of reacting efficiency detected under such conditions is undoubtedly important, it was our purpose in the present investigation to plan the experiments in such a way that they would measure only those effects from which the driver does not immediately recover. It was thought that transient changes might be less indicative measures of human efficiency and safety, and subject to more error in measurement, than those from which a driver might not quickly recover.

The present research indicates that the effect of long hours of automobile driving is a loss of efficiency of human reactions similar in principle to those required in driving, and may thus result in temporary accident proneness.

This view finds support in the literature in studies that have been made on the spoiled work of industrial operations; industrial accidents and output; and in studies of the effects of loss of sleep.

The hourly record of the percentage of spoiled work in certain industrial operations provides a running measure of the efficiency of the reacting mechanism of the worker as it is affected by hours of work. Spoiled work may be regarded as an accident to product or to the instrument handled. The hourly curve of spoiled work, then, constitutes a performance test that measures the changes of efficiency of the precise neuro-muscular reactions employed in the work in terms of the accidents to product resulting therefrom. The data obtained by Florence and Ryan indicate that in 10 hours of work at grinding springs the average percentage of spoiled work for the last two hours of the day was 14.00% greater than the average percentage of spoiled work for the first three hours of the day.[15]

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[14] Cf. footnote 4, page 404.

[15] P. S. Florence, *Economics of Fatigue and Unrest*, 1924, 268-271, 351.

Florence studied hourly output and hourly accident rates of several dextrous operations in the same ten-hour plant.[16] His data show that the average hourly output for the last two hours of the day in such operations was 11.32% less than the average hourly output for the first three hours of the day. The average ratio of accidents to output in the last two hours was 34.1% greater than the average ratio of accidents to output in the first three hours. In industrial operations, then, we have objective evidence of loss of efficiency of the reacting mechanism of the workers resulting from the work, associated with an increase in accidents.

In studies on the effects of loss of sleep on the human reactions, some of the tests employed in the present research were used on *Ss* deprived of sleep sufficient to render them probably unsafe as drivers in the light of practical experience.

Laslett obtained an increase in the body sway of 5.6% following a reduction of sleep of 40% on 5 nights; and an increase of 42.8% following a loss of sleep of 72 hr.[17] The former figure is less than the average increase in body sway for all of our driving days, while on many driving days the percentage increase of sway was greater than the latter figure.

Weiskotten found throughout a 62-hr. period of insomnia an average increase of 30.61% in the time required to add a given set of figures.[18] We found an average increase of 8.46% in the time required for mental addition on all driving days; and on a fifth of our driving days, the increase in time required for the test ranged from 19% to 61%.

Lee and Kleitman found an increase of 3.19% in time, and 41.54% in errors on the color naming test after a loss of sleep of 60-90 hr.[19] In our driving tests, the average increase of time for all days was 3.93%, with an average increase in errors of 46.79%; while on 15 days the increase in time required for the test was over 10%; and on 19 days the increase in errors amounted to 100% or over.

The importance of speed and accuracy of reactions in the safety of driving is obvious when one considers the distance an automobile will travel during the time required for the driver to react and to correct an error of reaction.

Summary and Conclusions

(1) There is a demonstrable fatigue effect of a long automobile drive on body reactions.

(2) Long automobile drives tend to decrease the fading time of the vascular-skin reaction; increase unsteadiness in standing; decrease the accuracy of hand-eye coördination; decrease the visual efficiency; decrease the speed and accuracy in naming colors; and decrease the speed and accuracy of mental addition.

(3) Greater variability occurs within the test scores made after long

[16] J. Goldmark and M. D. Hopkins, Studies in industrial physiology: Fatigue in relation to working capacity, *U. S. Pub. Health Bull.*, No. 106, 1920, 110.

[17] H. R. Laslett, Experiments on the effects of the loss of sleep, *J. Exper. Psychol.*, 11, 1928, 370-396.

[18] Footnote 9, p. 407.

[19] Footnote 8, p. 406.

automobile drives than before them in postural steadiness, hand-eye co-ordination and mental addition.

(4) The tendency of long automobile drives is to produce a loss of effectiveness of certain sensory discriminations, association processes, and motor reactions similar to those required in driving. These observations suggest that the effect of a long automobile drive may render a driver temporarily prone to accidents.

ELECTRIC CURRENT THRESHOLDS AT AUDIO-FREQUENCIES

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Psychopathic Hospital, Iowa City, Iowa

The present paper reports a study of alternating electric-current thresholds and of the relation between the frequency of the current and fatigue, adaptation, and recovery.

The twenty-two adult individuals who served as *O*s were either graduate or undergraduate students in the university. The two sexes were approximately equally represented.

Apparatus and Method

One stimulating circuit contained a General Radio audio-frequency oscillator, Type 377-B, a suitable attenuator, a high quality amplifier, a calibrated vacuum-tube voltmeter, a General Radio decade resistance-box and a pair of electrodes (Fig. 1). The other stimulating circuit was identical with the first except that the source of the stimulating sinusoidal current was a General Radio beat-frequency oscillator,

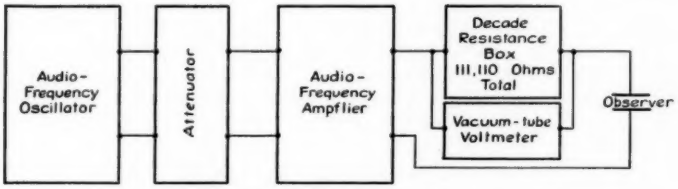

Fig. 1. Block Diagram of the Stimulating Circuits

Type 613-B. Attenuators were used in order to vary the voltage output of the oscillators over a wider range. We used amplifiers to increase the range of voltage available and to avoid placing *O* directly in the output circuit of the oscillators. Since we were using a resistance in series with *O* to determine thresholds, the amplifiers were necessary to give sufficient range of voltage for the determination of threshold values at all frequencies studied. The need of extra voltage occurred especially with high frequencies. Since a low range alternating current milliammeter for the measurements of the stimulating current was not available, we used a resistance in series with *O* and measured the drops in voltage across the resistance. For this purpose we employed vacuum-tube voltmeters which drew no current from the circuits across which they were connected.

O was placed in the circuit by inserting his forefinger in one electrode-bath and his second finger of the same hand in another electrode-bath. The electrode-baths

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(glass receptacles) contained concentrated saline solution. The electrodes were copper strips which gave a contact surface with the saline solution of 42 sq. cm. The electrode impedance was negligible as compared with the total resistance in the circuit.

The first phalanx of each finger was immersed in the saline solution. The median nerve supplies the sensory fibres to the skin covering the first phalanx of the first two fingers.

The threshold values were read in terms of volts. The voltage was divided by the resistance in series with the fingers (Ohm's law). Thus the thresholds were expressed finally as current in milliamperes.

It must be emphasized that electrical thresholds are reported in terms of current. We learned that it was useless to study the electrical thresholds in terms of voltage because of certain uncontrollable factors in the circuit, such as the changes in impedance with changes in frequency and different impedances for different *O*s at the same frequency. In general the impedance was lowered as the frequency was increased. We were unable to determine exactly why this was so. Fricke[1] and Fricke and Morse[2] made an analysis of the impedances of the blood and found that they constituted an extremely complex system of resistances and capacitances. Such a system might conceivably operate in the skin and underlying tissues. A cell in the skin or in underlying tissues undoubtedly has a capacitance-effect in relation to adjoining cells. Since the impedance of a capacitance varies inversely as the frequency, we would have a small impedance for high frequencies and a large impedance for low frequencies. Probably one *O* differed from another *O* mainly in terms of skin resistance. It is conceivable, however, that individual differences may have been due to the differences in the relationships between the skin and the underlying tissues. In view of these facts it is evident that, although we might have a constant voltage input to *O*, the current would vary for each frequency used and would change from *O* to *O*. The only thing left for us to do was to measure the current passing through the fingers.

Since the fingers formed part of the path through which the current flowed, it was obvious that we could compensate for any losses in other parts of the circuit by increasing the output voltage of the amplifier. Furthermore, since the current in a series circuit is everywhere the same, a measurement of this current gave us an adequate index as to the strength of the stimulus. The most convenient way to measure the small currents encountered and to take care of the wide range of variation with frequency was to use a variable decade resistance in series with *O* and a vacuum-tube voltmeter connected across the resistance. The voltmeter measured the voltage drop. The current is the voltage divided by the resistance. These resistances were very accurate and the vacuum-tube voltmeters were checked at frequent intervals against alternating-current voltages of known accuracy. The voltmeters were of the Class B, biased-to-cutoff type, which gave increases in plate current for increases in input voltage. Inasmuch as both the calibrating voltage and the audio-frequency voltages used in the experiments were sinusoidal in character, there was no error in the voltmeter calibration, such as would be the case if the

[1] H. Fricke, The electric capacity of suspensions with special reference to blood, *J. Gen. Physiol.*, 9, 1925, 137-152.

[2] Fricke and S. Morse, The electric resistance and capacity of blood for frequencies between 800 and 4½ million cycles, *ibid.*, 153-167.

second harmonic content were large and the voltmeter leads reversed. What we measured was the actual flow of current through the fingers.

Results and Discussion

Sensitivity of the finger tips to electric currents. The determination of absolute thresholds was accomplished by two methods: (1) a method in which the strength of the stimulus was altered continuously until *O* announced his perception of it; (2) a method in which various values of a stimulus were separately submitted until *O* announced his perception of it. Moreover, with both methods, the order of succession of the stimulus values was both ascending (T_u) and descending (T_l). The difference between T_u and T_l may be termed the threshold band. This band was

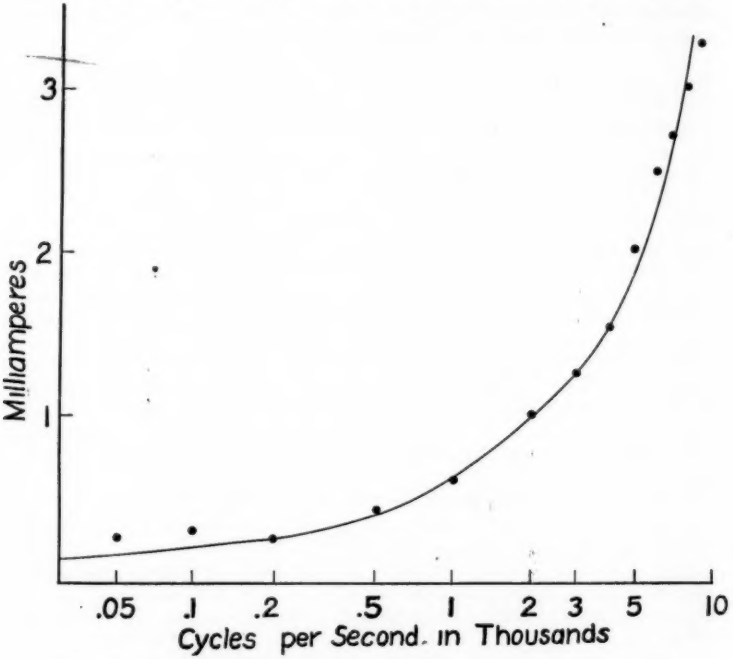

Fig. 2. Electric-Current Threshold Curve with Threshold Values Plotted on Semi-Logarithmic Coördinates

The curve has the same form regardless of whether the strength of the stimulus is altered continuously or the various values of the stimulus are separately submitted.

rarely more than 0.01 milliamp., which at all frequencies was a small fraction of the threshold value. When the limen had been determined reliably for any given frequency, an adequate rest period was allowed *O* before the limen for another frequency was determined.

Fig. 2 shows the threshold curve for the various frequencies studied. Generally speaking, the higher the frequency, the larger the amount of current necessary to produce a sensation. The curve, however, does not begin to rise until after 200~, being practically flat between 50~ and 200~.

The curve for the electric-current threshold affords interesting comparisons with the functions for touch (mechanical vibrations) and for hearing, reported by Goodfellow[3] and Fletcher[4] respectively. In general, the sensitivity of the finger tips to mechanical vibrations decreases with increased frequency. It remains relatively constant for frequencies of 64, 128, and 256, but decreases more or less gradually for successively higher frequencies up to 8192. The curve for the threshold of tactual vibration is, therefore, practically identical in form to the curve for the threshold of electric current. The curve for hearing shows greater sensitivity for frequencies in the middle range and lesser sensitivity for both the lower and higher frequency ranges. It differs strikingly from the curves both for the threshold of tactual vibration and that of the electric current.

The similarity between the curves for these two thresholds of tactual vibration and electric current may be explained by the fact that although the stimuli are different the neurological mechanisms are the same. It is reasonable to suppose that the electric current at the threshold level stimulated the touch, rather than the pain or thermal, receptors since the former have a lower threshold than the latter.[5] The introspective reports of the *O*s lend support to this contention. The *O*s never reported pain or temperature sensations; they did report sensations of vibration and tingling.

The difference between the curve for hearing and the curves for mechanical vibrations and electric currents may be due to differences in sense-organ adjustment to environmental demands. Sense-organs for touch were developed biologically in relation to the relatively low frequencies of mechanical vibrations, while sense-organs for hearing were developed in relation to the relatively frequently occurring middle range frequencies of air vibrations set up by sound sources.

The finding that the higher the frequency the larger the amount of current neces-

[3] L. D. Goodfellow, The sensitivity of the finger-tip to vibrations at various frequency levels, *J. Franklin Inst.*, 216, 1933, 387-392.

[4] H. Fletcher, *Speech and Hearing*, 1929, 157.

[5] P. Heinbecker, G. H. Bishop, and J. O'Leary, Pain and touch fibres in peripheral nerves, *Arch. Neurol. & Psychiat.*, 29, 1933, 771-789.

sary to produce a sensation may be accounted for on the basis of a function of the stimulating current rather than differences in the responsive characteristics of the sense-organs. By "function of current" we refer to the well known electrical phenomenon of "skin effect." The *skin effect* is the fact that with higher frequencies the current is almost entirely confined to the surface layer of the conductor. The central parts of the conductor do not carry any appreciable part of the current at high frequencies. If the skin effect operates, it is possible that the apparent decrease of sensitivity of the fingers to higher frequencies is due to the greater amount of current necessary to penetrate sufficiently to reach the sense-organs. Since the skin effect has

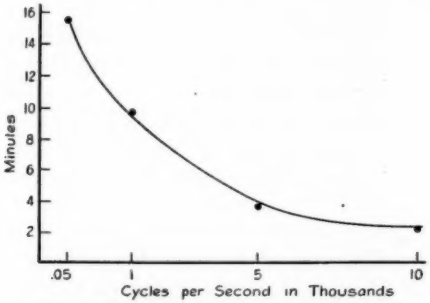

FIG. 3. MEAN FATIGUE-ADAPTATION CURVES

been thoroughly investigated only in relation to metallic conductors, our explanation of the threshold curve on that basis is merely tentative. We feel, however, that at the present time it is difficult, if not impossible, to tell how much of the differences in thresholds between higher and lower frequencies is due to differences in sense-organ responses and how much to skin effect.

Fatigue-adaptation effects. Particularly in working with the higher frequencies we noted that when there was some delay in determining the threshold with the continuous current method, the threshold value was higher than when it was determined relatively quickly. Thus, it appeared that fatigue-adaptation effects were operating. To investigate this possibility we ran two sets of experiments. In the first one we determined how long it took *O* to become unaware of the electric current when it was applied continuously at double the threshold value at a given frequency. Results of this experiment with five *O*s are given in Fig. 3. This figure shows a consistent decrease in the length of time required for *O* to become unaware of the continuously applied stimulating current in going from lower to higher frequencies. In other words, the higher the frequency the sooner *O* becomes fatigued by, or adapted to, the current.

In the second experiment, we utilized two stimulating circuits, one for the right hand and the other for the left hand. The thresholds were determined for both hands and a current at double the right hand's thresh-

old value was passed continuously through the right hand.[6] At intervals of 2 min. the current from the second stimulating circuit was passed through the left hand at an intensity which *O* judged to be equal to the strength of the current appearing at that moment in the right hand. Typical results of this experiment are shown in Fig. 4. These curves are for one *O* only, since it was impossible, on account of individual differences in respect of the operation of fatigue-adaptation effects, to obtain averages. These curves, however, are of the same general form as those obtained for five other *O*s. For all frequencies it may be noted that there is a relatively rapid

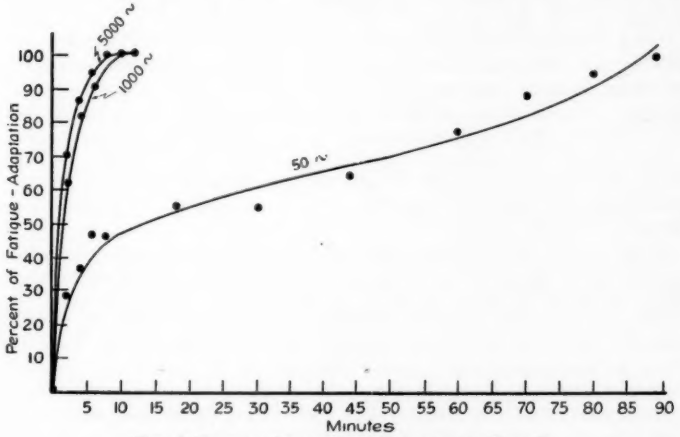

FIG. 4. FATIGUE-ADAPTATION CURVES FOR ONE *O*

initial decrease in sensitivity, with the dulling effect proceeding at a slower rate toward the latter part of the fatigue-adaptation period. This characteristic of the fatigue-adaptation effect is generally more conspicuous for the higher than for the lower frequencies. Significant deviations from the typical curve relationships presented in Fig. 4 occurred. The curve for 1000~ may rise more slowly than that for 50~; the 1000~ current may cease to stimulate *O* at a value considerably above the original limen; and the 50~ curve may rise in steps.

[6] When the thresholds for the two hands were not equal, all values for the left hand were multiplied by a constant to make them equal to the values for the right hand. Differences in the thresholds for the two hands cannot be readily accounted for. Since thresholds were measured in terms of current, differences in resistance of the two hands cannot be responsible. It is possible that for certain *O*s at certain frequencies there is a genuine difference in the sensitivity of the two hands. This phase of the study needs to be investigated further.

Similar to the above results are those reported by Smith on audition.[7] In studying auditory fatigue, he found greater fatigue effects at the higher (above 1000 ~) than at the lower test frequencies.

Adrian found that the frequency of the discharge of a sense-organ declines under a constant stimulus applied to the sense-organ.[8] In our experiment this would mean probably that, as the electric current continued to stimulate the sense-organs in the finger tips, the frequency of sense-organ discharges dropped. Adrian[9] also found that an increase in the intensity of the stimulus produced an increase in the frequency of sense-organ discharges. Reducing the frequency of sense-organ discharges would conceivably decrease the intensity of the sensation. Thus, we have a physiological explanation of the psychological fact that, as we continued to apply the current at a given intensity and frequency to the finger tips, *O* reported decreasing intensity of sensation until the sensation finally entirely disappeared.

We have no ready explanation of the fact that the decrease in the intensity of the sensation was more rapid at first. Adrian,[10] however, reported that the frequency of sense-organ discharges declines more rapidly at first than later when the pressure stimulus is constant. Bronk has shown that true fatigue effects most probably occur towards the end of a long discharge from a tension receptor,[11] but the initial decline is evidently due to a direct adaptation to the stimulus. If this is true and since adaptation probably plays a much larger rôle than fatigue in producing the total fatigue-adaptation effect, it is conceivable why the decrease in the intensity of the sensations was more rapid at first.

If the sense-organs stimulated by the electric currents were developed biologically in relation to relatively low frequencies, it is understandable why fatigue-adaptation effects operate more slowly for the lower than for the higher frequencies. The stimulated sense-organs respond more efficiently at the lower frequencies, with the result that they fatigue or become negatively adapted less readily when exposed to slower environmental changes. Smith's explanation of greater auditory fatigue at the higher frequencies may hold as well for the similar results reported here for electric-current stimulation.[12] He states that if the effect of the fatiguing stimulus were to lengthen the refractory period of nerve-fibers or synapses, according to the volley theory of Wever,[13] this would lead to a decrease in the effective number of working fibers, which would in turn lead to a decrease in the sensitivity of the hearing mechanism.

Recovery from fatigue-adaptation effects. Recovery from fatigue-adaptation effects was treated in two ways. First, we determined for each of five *O*s how long it took *O*, after he had become unaware of a continuously applied current at double the threshold value, to regain his original sensi-

[7] A. F. R. Smith, Auditory fatigue, *Brit. J. Psychol.*, 25, 1934, 77-85.
[8] E. D. Adrian, *The Basis of Sensation*, 1928, 65-66.
[9] *Op. cit.*, 93-94.
[10] *Op. cit.*, 114-115.
[11] D. W. Bronk, Fatigue of the sense organs in muscle, *J. Physiol.*, 67, 1929, 270-281.
[12] Smith, *loc. cit.*
[13] E. G. Wever, The physiology of hearing: the nature of response in the cochlea, *Physiol. Rev.*, 13, 1933, 400-425.

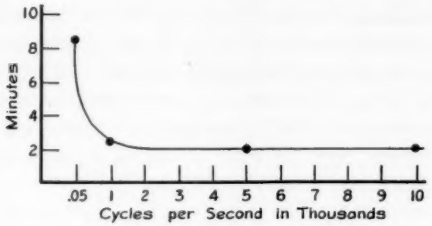

FIG. 5. MEAN RECOVERY CURVE

tivity. During the rest-period following the complete fatigue-adaptation period, *O*'s threshold was determined at 2-min. intervals. The results of this method of studying recovery are presented in Fig. 5. The curve shows the mean length of time for the five *O*s to recover their original threshold values for the four frequencies studied. It may be noted that, in general, the lower the frequency the longer the recovery period.

Secondly, the recovery curves were plotted for each *O* as presented in Fig. 6. It was impracticable to obtain averages. However, the curves in Fig. 6 for one *O* are characteristic of those for the other four *O*s. For all frequencies there is a rapid initial recovery, with the restoration of the original sensitivity proceeding at a slower rate toward the latter part of the recovery period. This characteristic of the recovery phenomenon is more

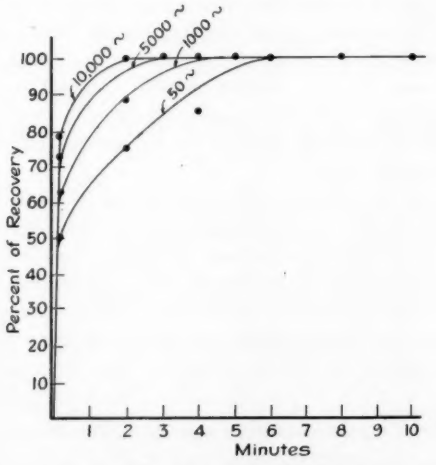

FIG. 6. RECOVERY CURVE FOR ONE *O*

strikingly demonstrated for the higher than for the lower frequencies.

Adrian found that the nerve recovers its normal excitability much more rapidly during the earlier than during the later part of the recovery period.[14] This difference is not so strictly true of the end-organ which presents throughout the recovery period a more gradual return of its original excitability. Our recovery curves are more similar to Adrian's curves for nerve than to his functions for sensory end-organs.

The explanation of the fact that it requires less time for complete recovery to take place for the higher than for the lower frequencies is best given in terms of the analysis made in the next section. There it is argued that fatigue plays a proportionately smaller rôle as we progress from the lower to the higher frequencies while just the reverse of this is true of adaptation. If this is so, then the sense-organs recovering more quickly from adaptation than from true fatigue effects display an earlier restoration of their original sensitivity for the higher than for the lower frequencies.

The ratio of fatigue to adaptation. An attempt was made to determine how much of the fatigue-adaptation effect was due to fatigue and how much of it was due to adaptation. Adrian views fatigue as "a change consequent on the activity of the organ as distinguished from one directly consequent on the stimulus and independent of the amount of activity it has evolved."[15] Myers holds that fatigue arises from the exhaustion of material stored up in living cells for the maintenance of their special functions, while adaptation involves an equilibrium between antagonistic sets and results in a posture.[16] From these statements it appears that for fatigue the organ or organism becomes exhausted by the stimulus, whereas for adaptation it simply becomes adjusted to the stimulus. In terms of these considerations we attempted to differentiate between fatigue and adaptation in our study of fatigue-adaptation effects.

An analysis of the fatigue-adaptation effect is given in Fig. 7. These curves were obtained in the following manner. As soon as *O* no longer 'felt' the current, the oscillator was switched suddenly to zero intensity. Immediately following this maneuver *O*'s threshold was determined. The difference between this re-determined threshold value and the amount of current used to produce the fatigue-adaptation effect (twice the original threshold value) was considered as due to adaptation. The difference between the

[14] Adrian, *op. cit.*, 63.

[15] E. D. Adrian, The mechanism of the sense organs, *Physiol. Rev.*, 10, 1930, 336-347.

[16] C. S. Myers, Conceptions of fatigue and adaptation, *Psychol. Rev.*, 32, 1925, 1-16.

re-determined threshold value and the original threshold value was considered as due to fatigue. The curves show that adaptation plays a proportionately larger rôle as we progress from the lower to the higher frequencies. The ascent of the adaptation curve is much more rapid than that of the fatigue curve. Expressing the relationship between fatigue and adaptation

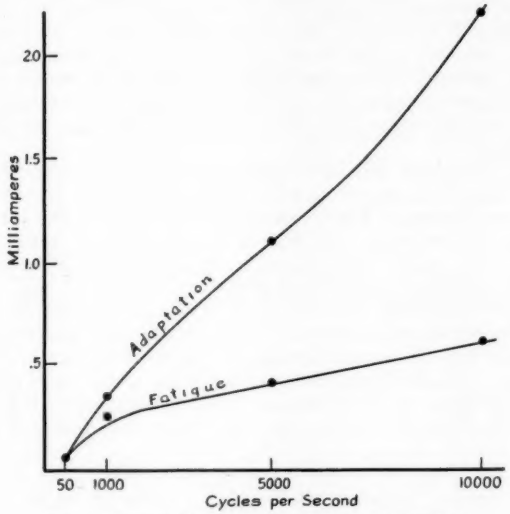

FIG. 7. CURVES SHOWING THE RATIO OF FATIGUE TO ADAPTATION

in terms of ratios (fatigue is to adaptation) we have the following statements for the *O* whose curves are presented in Fig. 7.

$$50 \sim = 1:1$$
$$1000 \sim = 1:1.7$$
$$5000 \sim = 1:2.7$$
$$10000 \sim = 1:3.7$$

For the four other *O*s there were no exceptions to the trends just noted for this one *O*.

On the basis of the work of Adrian it is not surprising that we found adaptation to be a more potent factor in causing the decline in sensation.[17] Adrian states that, although true fatigue of an end-organ is a possibility which cannot be altogether ruled out, there is no doubt that adaptation is

[17] Adrian, *The Basis of Sensation*, 1928, 67-68.

the more important factor in causing a decline in the frequency of sense-organ discharges and probably a consequent decline in the intensity of sensation.

Changes in sensitivity. For three of the six *O*s (fatigue-adaptation experiment) it was noted that, when the right hand was being continuously stimulated, the threshold of the left hand was lower than the original threshold of either hand. For one *O* it was lower with 50 ~ and 1000 ~ ; for another *O* with 50 ~ and for the third *O* with 50 ~ and 5000 ~ . Thus, it may be noted that for each of these three *O*s there was a lowering of the threshold for the 50 ~ . Hartmann found that visual acuity can temporarily be increased by the simultaneous application of auditory, olfactory and cutaneous stimuli.[18] It is possible that the increased sensitivity of some of our *O*s to certain frequencies might be similarly explained by other simultaneous stimulations.

Effect of skin lesions upon the threshold. With a limited number of *O*s, it was found that lesions in the skin (hang nails or punctures) tended to lower the threshold for the lower frequencies. The introspective reports indicated that the lower frequencies (below 500) were concentrated at the lesion, whereas higher frequencies were spread more or less evenly all over the finger-tip area. This phenomenon may be referred to the electrical phenomenon of skin effect. Since at the higher frequencies the current would be confined mainly to the surface area, the reduction in the resistance offered by a skin lesion would not cause a more ready passage to the interior for the higher frequency currents. Consequently the threshold would not be lowered. But for lower frequencies the opposite of this would be true, since those currents are carried mainly by the central portions of the conductor and the reduction in the resistance offered by a skin lesion would cause a more ready passage to the interior for them.

Skin lesions introduced another disturbing factor. Occasionally, they were the source of extraneous sensations which tended to confuse *O* in his judgment of whether or not the electric current was 'felt.'

Summary

The present paper reports a study of alternating electric-current thresholds at audio-frequencies and the relation of fatigue, adaptation and recovery as functions of the frequency of the stimulating current. Twenty-two adults served as *O*s. The following results were obtained.

(1) The threshold curves are practically linear, *i.e.* the higher the frequency the larger the amount of current necessary to produce a sensation. However, the curves do not begin to rise until after 200~, being practically flat between 50~ and 200~.

(2) There was a consistent decrease in the length of time required for *O* to become fatigued by, or adapted to, a continuously applied stimulating current in going from lower to higher frequencies.

[18] G. W. Hartmann, II. Changes in visual acuity through simultaneous stimulation of other sense organs, *J. Exper. Psychol.*, 16, 1933, 393-407.

(3) For all frequencies it was found that there was a relatively rapid initial decrease in sensitivity with the dulling effect proceeding at a slower rate toward the latter part of the fatigue-adaptation period.

(4) In general it was found that, the lower the frequency, the longer it took *O* to recover from fatigue-adaptation effects.

(5) For all frequencies there was a rapid initial recovery, with the restoration of the original sensitivity proceeding at a slower rate toward the latter part of the recovery period.

(6) The higher the frequency, the greater the rôle played by adaptation.

(7) For some *O*s at certain frequencies it was noted that, when the right hand is being continuously stimulated in the fatigue-adaptation experiment, the threshold of the left hand is lower than the original threshold of either hand.

(8) It was found that lesions in the skin tend to lower the thresholds for the lower frequencies.

(9) Electrical threshold curves were compared with similar curves for touch and audition; and fatigue, adaptation and recovery phenomena were related to Adrian's findings on sense-organ discharges and sensory nerve-conduction.

SOME FACTORS INFLUENCING THE BRILLIANCE LIMEN OF VISION

By H. Birnet Hovey, University of Southern California

The present study was undertaken to discover whether the pattern of a visual field plays a rôle in the differential limen for brilliance. Specifically we sought to determine what effect, if any, configurational factors would have upon the brilliance limen. We found that such factors do have effects.

Method and Procedure

The *O*s, 5 in number,[1] were seated in a darkroom before a lighted screen. They observed sometimes singly and sometimes in groups of two or three. When singly, they were placed almost directly in front of the fixation-point; when in groups, they

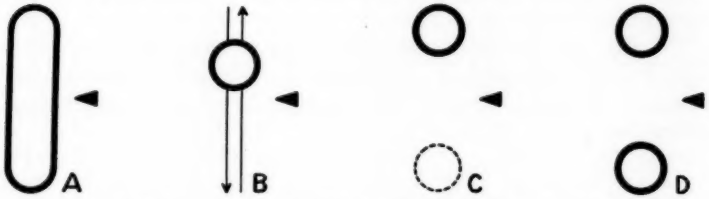

Fig. 1. Diagrams of Stimulus Configurations Used in the 6 Experimental Conditions

The triangle represents the point of fixation. The diameters of the circles and the width of the oblong represent a visual angle of 1°. All other relations are drawn to scale in proportion. (A) Pattern used in the *Stationary* and *Modulation* conditions. (B) Pattern and course for *Real motion*. (C) Pattern for the *Phenomenal motion* and *Alternation* conditions. The broken circle represents the alternate position. (D) Pattern for the *Flicker* condition.

were placed on the arc of a circle the center of which was at the fixation-point. The observation distance in every case was 7 ft.

The observation screen—a light gray, dull finish cardboard 32 in. × 38 in., was evenly illuminated excepting one spot, the shadow of a pointer, which served as the point of fixation. The illumination was produced by a 500-watt Bausch and Lomb balopticon with the amperage of the standard 110 volt electric current reduced by a rheostat to ¼th. This illumination was held constant, at about 3.2 ml., throughout the course of the experiment.

The comparison area—near the point of fixation—was of greater radiant intensity than that of the background. The illumination of this area was varied from

* Accepted for publication August 30, 1934.

[1] The *O*s were Walther Joël, W. Brant Clark, Robert Ruhl, Marion Warner Hovey and the author. The first three and the last were pursuing work toward the doctorate in psychology; the fourth, a biochemist, underwent a preliminary practice period of considerable length, helping to develop the adopted procedures.

0.002 to 0.024 of the background intensity in steps of 0.002. This light came from additional projectors photometrically adjusted to equal the output of the first lantern, the steps in intensity being regulated with an episcotister.[2]

The experiment consisted in determining by the method of constant stimulus differences, the differential limens for brilliance for every one of the following six conditions.

Condition 1. (Stationary). About ½ sec. before an observation, the comparison-stimulus was presented on the screen in the form of an oblong figure as in Fig. 1A below. *O* refrained from looking at the screen until given a signal to begin the trial. He then fixated for 3 sec., at the end of which time he was given another signal to glance away. After the trial he recorded an "S" or an "O," according to whether he believed there was a comparison-stimulus presented during the trial. The area of the oblong figure, when actually presented, was $4° \times 1°$ visual angles.

Condition 2. (Modulation). This was much like the condition called *Stationary* excepting that *O* was fixating while the comparison-stimulus was thrown on the screen and removed, permitting his eyes to be exposed to the transition from the level of the background intensity to an increase in intensity within the boundaries of the figure, and back again to the original level of the background.

Condition 3. (Real motion). This consisted in a circular area of illumination moving up and down in simple harmonic motion within the outline of the oblong area prescribed for the two preceding conditions. It oscillated a little faster than twice per sec. The stimulus is diagrammed in Fig. 1B. The effect was produced by rotating a wheel to which a lantern slide was attached.

Condition 4. (Phenomenal motion). Two circular areas in the end positions of the oblong were presented alternately. *O* watched for the appearance of movement on the screen.[3] The pattern is indicated in Fig. 1C. The intervals between the alternate exposures, and the exposures themselves, were timed to conform with optimum conditions for the *phi*-phenomenon, in accord with Korte's laws.[4] The pattern was produced by alternately interrupting the beam from each of two projectors. The interruptions were made by rotating, immediately in front of the two objective lenses, a large disc with two radial slots cut in it.

Condition 5. (Alternation). The presentation was identical with the preceding condition excepting for instructions to *O*. This time *O* paid no heed to the phenomenon of movement, but instead watched for the circles to appear and disappear.[5]

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[2] The reader may obtain detailed information on procedure, apparatus, and statistical treatment of data by consulting the doctoral dissertation which is in the University of Southern California Library at Los Angeles.

[3] Since the literature seemed to be lacking in accounts of work on phenomenal motion in the region of the limen for brilliance, it was necessary to determine whether the phenomenon could be obtained at all, around liminal brilliance before Condition 4 could be incorporated in the experiment. It was obtained. Optimum exposure time was found to be about 1⅞ that of time-interval; and the time-interval —for the stimulus with a value of 0.01—was about 1/6 sec.

[4] A. Korte, Beitrage zur Psychologie der Gestalt und Bewegungserlebnisse, *Zsch. f. Sinnesphysiol.*, 72, 1915, 193-296.

[5] The reason for using this condition is interesting. Instructions to *O* had been to guess at all times when he was in doubt about the stimulus actually being present during a trial. The first time the series of observations was made under the condition in which the two circles were presented alternately, no mention had been made on the experimenter's part for *O* to watch for phenomenal motion. Three of the *O*s

Condition 6. (Flicker). This was similar to the last two conditions explained excepting that the two circles were exposed simultaneously. Fig. 1D represents the pattern.

Two experimental periods were devoted to every one of the 6 conditions investigated; in one the steps in intensity were varied from high to low, and in the other from low to high. In order to equalize effects of practice, a program of periods was so arranged that no *O* would complete the observations for one condition before starting upon those of another. Effects of practice were further minimized by presenting to *O*, a large number of trials under every condition prior to the experiment itself.

In the experiment, every *O* was given 100 trials at every step, under every condition. The comparison-stimulus was actually presented 50% of the time and in random

TABLE I

MEANS OF THE SCORES IN PERCENTAGES

Intensity	Condition					
	1	2	3	4	5	6
0.002	−0.8	0.4	2.0	1.2	2.8	3.2
0.004	11.2	14.0	19.6	2.4	11.6	12.8
0.006	15.2	28.4	57.0	11.2	21.2	23.6
0.008	37.2	48.0	90.8	41.2	38.4	37.2
0.01	59.2	72.8	98.4	87.6	52.0	60.0
0.012	78.4	86.4	100.0	95.6	75.2	79.6
0.014	84.0	97.2		98.0	89.2	92.4
0.016	95.6	98.0		98.8	96.8	96.8
0.018	96.8	99.6		100.0	99.6	99.2
0.02	99.2	100.0			99.6	100.0
0.022	100.0				100.0	

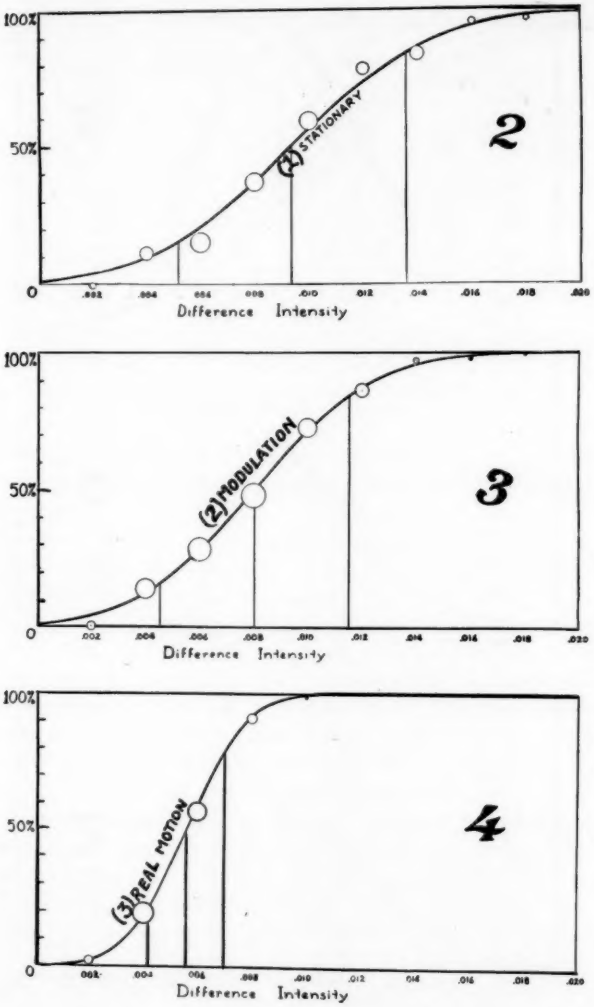
order. Before a set of trials at any step was given, the figure was shown for a few seconds at a value of 0.1, followed by a few 'free' trials at the step to be used, during which *O* knew the stimulus was present whether he could appreciate it or not. This was done to equalize orientation from one set of trials to the next, and to equalize it for all *O*s.[6]

RESULTS

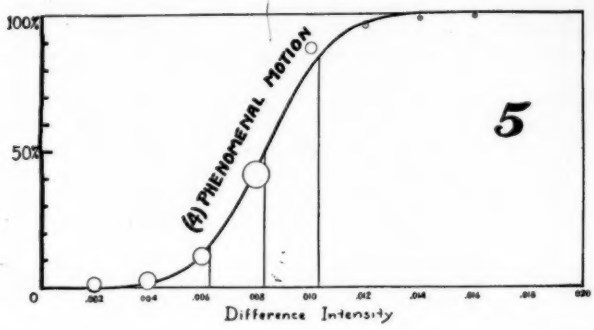
The results are based on comparison-stimuli ranging from 0.002 to 0.024, the latter being beyond the highest step on which any errors were made under any condition by any *O*. An *O* began a sitting at a step which was

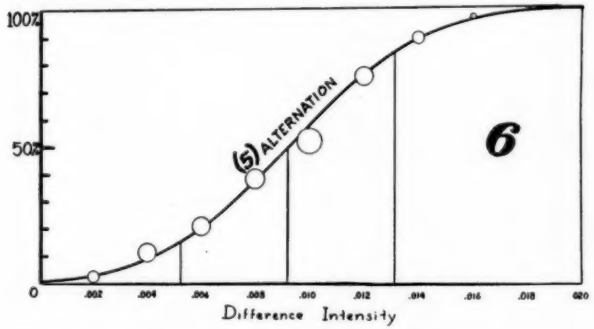
entirely ignored such motion as a cue, for they presumed it would be of no value in their guesses. They watched, instead, for appearance and disappearance of one of the circles, notwithstanding the fact that all *O*s had been well tested for perception of phenomenal motion at liminal brilliance prior to beginning the experiment. These *O*s were therefore asked to attempt to use phenomenal movement alone as the cue in a second series of observations. They found this easy to do, and found it easy to ignore appearance of the circles. The other two *O*s had been using the movement cue from the outset, so they were asked to change in the second series, and use appearance of a circle as the cue. The results of every *O* were recorded under Condition 4 or Condition 5 according to the mental set used while making the observations.

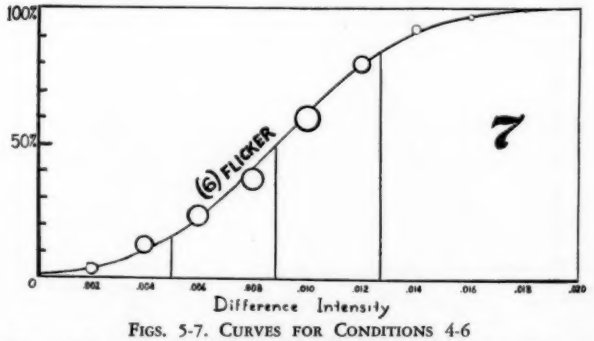
[6] This orientation practice counteracted the expected difference in the limen when measured by an ascending and by a descending order of steps.



FIGS. 2-4. CURVES FOR CONDITIONS 1-3
The middle vertical line crosses the threshold, and the two lines on either side cross the standard deviations.







FIGS. 5-7. CURVES FOR CONDITIONS 4-6

The middle vertical line crosses the threshold, and the two lines on either side cross the standard deviations.

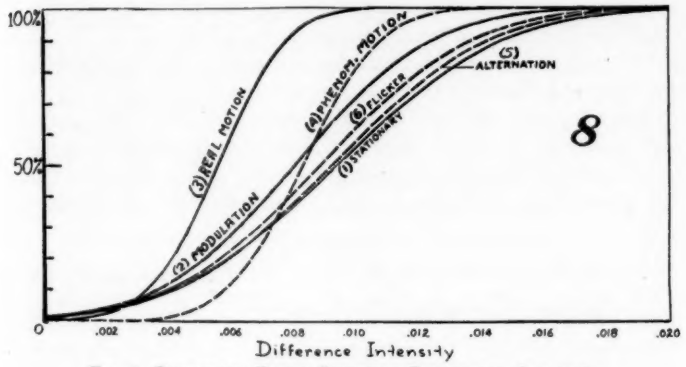


FIG. 8. COMPOSITE GRAPH SHOWING CURVES FOR ALL THE EXPERIMENTAL CONDITIONS

known to be high enough for him to score perfectly, or, if he started at 0.002 he continued to, or a little beyond, the step at which he scored perfectly.

Because of the lack of space, separate treatment of the data of the different *O*s is not reported. Suffice it to say that inspection of the scores showed similar trends for all *O*s. Therefore only coefficients expressing group results are given. A complete table of means for observations under every one of the six conditions is given below. The lefthand column lists the steps in intensity above that of the background and the other columns are mean percentages of the correctness in perception under the six conditions.

A better picture of the data can be obtained from the graphs shown in Figs. 2 to 8 inclusive. The *x* axes show the values of the steps in intensity. The *y* axes represent the percentages of correctness at the steps. Probable errors are shown by circles, the diameters of which are equal to the distances of these errors from the means along the *y* axes. Every percentage in Table I and every point on the six graphs represents 500 trials. The graph for Condition 1, for instance, is based on 5500 trials.

The points on the graphs produce ogive or inflexion curves—which appear to change direction of curvature in the regions of the 50% points on the *y* axes. Slope varies from one curve to another, as seen in Fig. 8, showing that the spread of the data varies according to the condition used for presenting the stimuli. Kelley describes a method for determining the midpoints of such ogive distributions and their standard deviations.[7] His

[7] T. L. Kelley, *Statistical Method*, 1924, 326-330.

principles were followed in computing these coefficients for the six distributions. The coefficients are shown in the table below.

The shapes of the experimental curves suggested that they were all of the same type. This was found to be true. The data in every one of the six series followed the integral of a normal probability distribution. This type of equation gave the closest fit available for the six sets of data. The curves computed from this equation are shown in Figs. 2 to 8 inclusive. The Kelley-Wood Table was almost indispensable when calculating these

TABLE II

MIDPOINTS AND STANDARD DEVIATIONS FOR THE BRILLIANCE LIMEN UNDER THE 6 CONDITIONS

Condition	Midpoint ($\bar{S}$)	S.D. (σ)	$\bar{S}-\sigma$	$\bar{S}+\sigma$
1) Stationary	.0094	.0042	.0052	.0136
2) Modulation	.0080	.0035	.0045	.0115
3) Real motion	.0056	.0014	.0042	.0070
4) Phenomenal motion	.0083	.0020	.0063	.0103
5) Alternation	.0092	.0039	.0053	.0131
6) Flicker	.0089	.0038	.0051	.0127

curves.[8] Since the curves pass within nearly all the P.E. of the means, and since every point on any one of the curves is based on all of the means in the empirical series, we have assumed that the curves obtained are better representations of the experimental results than are the means themselves.

The curves are similar to those obtained by Urban when he used standard and comparison-weights.[9] Thurstone has criticised such curves by pointing out that the empirical curve from plus-minus judgments can never be exactly identical with the theoretical, since threshold values below the constant stimulus are not comparable to those above; that the empirical curves would always be skewed.[10] His criticism probably would not apply to our results because all the comparison-stimuli were in the same direction from the standard. The equation nevertheless can be only a close approximation, and not an exact expression of our data, because its curve begins on the base at $-\infty$ instead of somewhere above zero on the x axis.

DISCUSSION

Since any one of the six sets of data tends to follow the integral of a normal distribution, the limen for brilliance does not follow the all-or-

[8] Kelley, *op. cit.*, 373-385.

[9] F. M. Urban, The method of constant stimuli and its generalizations, *Psychol. Rev.*, 17, 1910, 229-259.

[10] L. L. Thurstone, The Phi Gamma hypothesis, *J. Exper. Psychol.*, 2, 1928, 293-305.

none principle. An hypothetically basic limen is approached asymptotically rather than directly; an upper limit beyond which perception is invariably clear is also approached asymptotically.

That intensity at which perception takes place 50% of the time was given by Fechner as the limen for brilliance, and this has been accepted ever since. The fact that our data yielded curves with varying slopes shows that the 50% point does not in itself present a picture of the limen. Giving the limen this designation is doing nothing more than defining the point above and below which it is equally apt to fluctuate. One σ below the midpoint ($\bar{S} - \sigma$ in Table II) might be considered as a threshold value, for instance, and have as great representative significance. It suggests the region of intensity where perception has begun to occur. One σ in the other direction ($\bar{S} + \sigma$) may be considered representative as it points out the region where errors are approaching a minimum. Such considerations indicate that the limen can be adequately expressed only in terms of the curve of distribution.

An example of a situation in which the midpoint is inadequate can be seen by comparing Curves 4 and 6 in Fig. 8 above, and by comparing the midpoints (S) with their σs in Table II. If the $\bar{S}$s alone were compared, the limen under Condition 4 *(Phenomenal motion)* would be lower than that under Condition 6 *(Flicker)*. If the lower σs alone were compared, a reverse relationship would obtain, the σ for the latter being nearer zero difference in intensity. Another example is found in comparing Curve 4 with Curve 2. Here $\bar{S}$ on the latter is lower and the upper σ is higher, than the corresponding points on Curve 4. Comparison of Curves 2 and 3 provides a third example. The difference between their Ss is about 8 times the difference between their lower σs. The importance of curves for expressing the limen having been pointed out, we can now proceed to a discussion of the limen as measured under different configurations of the stimuli.

Attention is first invited to a comparison of the curves for Conditions 3 and 1 (*Real motion* and *Stationary*). Under both conditions a surface $1° \times 4°$ on the semispherical retina was stimulated by identical differences in intensity in two ways, yet the curves are radically different.

A better comparison of method may be had by inspecting the superimposed curves for Condition 3 and 2 (*Real motion* and *Modulation*). Both conditions were the same excepting for the fact that in one, successive portions of the retina within the image of the figure were stimulated progressively instead of simultaneously. Under both conditions every point within the image was stimulated by an increase in intensity from that of

the background to that of the comparison-stimulus and then by a reduction in intensity to that of the background again. The limen under Condition 3 (*Real motion*) rises toward its maximum more rapidly than it does under Condition 2 (*Modulation*). This difference can hardly be attributable to processes in the retina or optic tract.

Comparison of the curves of Conditions 4 and 6 (*Phenomenal motion* and *Flicker*) shows a most striking difference for the courses of the limen under the two conditions of stimulation. It would require a far-fetched assumption to explain this difference in terms of retinal processes. A pair of circular areas, having no special physiological connection in the retina or optic tract, were stimulated alternately under one condition and simultaneously under the other.

Conditions 4 and 5 (*Phenomenal motion* and *Alternation*) provide a comparison which could not be achieved in any of the other comparisons. Their curves have radically different slopes while the retinal conditions were identical in every respect. The only difference in the two methods was in the mental set of the *O*s. The comparative effects on the curve for the limen show, more distinctly than any of the other comparisons, how the limen may depend upon perception of a field pattern *per se*.

The phenomenon of motion in the configuration seems to have had a special effect on the limen. The curves for Conditions 3 and 4 (*Real motion* and *Phenomenal motion*) have greater slopes than any of the other four. This being true for the *Phenomenal Motion* curve suggests that real motion in the environmental pattern was not the factor producing the effect but rather central processes in the organism which were reacted to as motion. Real and apparent motion, however, did not have the same effects on the position of the limen. Real motion seems to have had in it something additional which caused a shifting of the whole limen to a lower position. *O* was possibly responding, under one condition, to motion together with a definite object, and to motion alone under the other.[11]

The six curves support Guilford's hypothesis that physiological factors within the organism help to make up the limen.[12] He showed that the chance disposition of factors facilitating and inhibiting perceptibility is ever changing—as the stimulus intensity is increased, fewer and fewer facilitating factors must of necessity be present at any moment for the stimulus to be perceived. At the midpoint of a distribution, let us assume with him,

[11] Attempts on the part of *O* to notice the circles in the *Phenomenal motion* series interfered with the phenomenon of movement.

[12] J. P. Guilford, Fluctuation of attention with weak visual stimuli, this JOURNAL, 38, 1927, 534-583.

that there would be a balanced disposition of the two types of factors; that the proportion of facilitating factors necessary for perception would have to be progressively greater as the intensity of the stimulus is decreased from this point—that larger and larger proportions of inhibiting factors will permit perception as the intensity is increased from this point. The chance disposition of ever increasing proportions follows the theory of diminishing returns. Our curves, therefore, could be expected to depart from their midpoints toward the base—and toward the upper limit, according to this theory, if his thesis is correct. They actually do. If variations in perception from trial to trial were due to fluctuations in attention, the curves should not follow the courses they do. Instead, they should begin at the base at the lowest effective difference in intensity and from there describe curves of diminishing returns. The empirical curves rise gradually at first, then more rapidly, and then gradually flatten. The inference then is that the type of curves described for the limen is due to ever changing physiological factors as Guilford contends.

These factors, however, would not account for the amount of slope in a given curve, nor would they necessarily determine the position of one on the x axis. The configuration of the stimuli seems to be involved. Thus we may, for the purpose of defining a concept of the brilliance limen, arbitrarily assume three categories of factors. The first would be the relative radiant intensities in the field and their concomitant optic processes. Ever changing physiological factors within the organism would be the second. The remaining one would be reaction to the temporal and spacial configuration of the field.

The present experiment throws some light on the causes of discrepancies in measurements of the brilliance limen made by various workers. Helmholtz was able, for instance, to regularly perceive a difference as small as 0.006,[13] while the results of König show no persistent values lower than 0.017.[14] The patterns of the fields these investigators used might have varied from one investigator to another. All their different measurements might have represented equally well controlled experimental conditions. A perfect experimental set-up for measuring the limen may not exist—even theoretically. The limen is perhaps always a function of all the experimental conditions rather than a simple function of intensities.

There arises the question of what were the actual stimuli which gave

[13] H. L. F. von Helmholtz, *Handbook of Physiological Optics*, tr. by Southall, 2, 1924, 179.

[14] A. König, Über die Anzahl der unterschiedbaren Spectralfarben und Helligkeitstufen, *Zsch. f. Sinnesphysiol.*, 8, 1920, 176-213.

rise to the responses of an *O*, or what might have been the physiological counterparts of his experiences of brilliance, in our experiment. Energy emitted by the objective field may have set up, in a field of cones and rods, photochemical gradients which were propagated to the cerebral cortex—and still there may have been no perception of the comparison-area in the objective field. There is good reason for supposing that neural impulses reached the cortex in like manner under Conditions 4 and 5 (*Phenomenal motion* and *Alternation*). Factors determining presence or absence of perception under these two conditions must have played their parts after the impulses reached the cortex. Possibly the perception itself was a definite reaction of the individual to the subcortical impulses, organized with other activity going on in him, rather than being a matter of the cortical sensory areas being in a condition for 'recognizing' the impinging impulses. These considerations lead to the conclusion that the limen for brilliance is controlled by some general pattern determined by all factors operative in the organism at the moment.

Conclusions

(1) The limen for brilliance seems to be, in part, a function of field-patterns, the relative radiant intensities within the field being only other factors entering into it, and not exclusive determiners of it.

(2) This limen can be adequately expressed only in terms of the curve of distribution of perceptibility as a function of stimulus-intensity.

(3) The limen tends to follow the course described by the integral of the normal distribution-curve, as the differences in intensity increase progressively above the standard stimulus. The position of the curve along the axis representing differences in intensity is apparently determined for the most part by processes in the afferent optic apparatus, and partially by *constant* facilitating and inhibiting physiological factors correlated with field configuration. The type of curve described seems to be due to the combined operation of *fluctuating* facilitating and inhibiting physiological factors. The amount of slope of a curve seems to be due to physiological factors specifically related to field configuration.

(4) The phenomenon of motion in the configuration, whether real or apparent, appears to have the special effect of increasing the slope of the curve.

(5) The average threshold for the perception of phenomenal motion is in the same order of magnitude as the thresholds for perception of differences in light intensity.

(6) The results support the contention that fluctuations in perception from trial to trial for a constant stimulus-difference is, instead of a function of attention, an actual shifting of the limen itself.

(7) We may conceive of the limen in terms of response, and not as a registration on the brain of afferent neural processes.

EXTERNAL INHIBITION AND DISINHIBITION PRODUCED BY DUPLICATE STIMULI

By M. A. WENGER, University of Iowa

The hypothesis that remote excitatory tendencies in serial learning are essentially similar to short trace conditioned reflexes has recently been advanced by Lepley.[1] Hull has further postulated that anticipatory intrusions in serial learning may be explained in terms of external inhibition and disinhibition.[2] An extraneous stimulation of appropriate intensity is conceived as externally inhibiting the immediate excitatory tendency and concurrently removing from the remote excitatory tendency the inhibition of delay which, according to Pavlov,[3] exists during trace and delayed conditional reflexes.

A similar condition obtains in Hull's explanation[4] of the experimental results of Valentine[5] in maze-learning by rats. Here an extraneous stimulus is conceived as inhibiting the habitual response at a maze-junction and removing, for another path leading toward the goal, the inhibitory tendencies which have recently been built up through repeated frustration. Here also both external inhibition and disinhibition are attributed to the same stimulus.

Analysis of these two assumptions reveals not only that the same stimulus must have opposite effects upon two responses, but that its effects must be such that the functional strength of the disinhibited response will be absolutely *greater* than that of the externally inhibited response; that is, there must be 'functional crossing.' In terms of Fig. 2 (p. 452, below), the action of an extraneous stimulus should produce a crossing of the lines representing the conditioned and the extinguished response respectively. Although Pavlov gave many examples of external inhibition and of disinhibition, he apparently presented no data which would assist in determining the tenability of Hull's assumptions. In fact, close inspection of Pavlov's two major

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[1] W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 279-288.

[2] C. L. Hull, *The Delayed Condition Reflex and Serial Learning*, (Unpublished MS).

[3] I. P. Pavlov, *Conditioned Reflexes: An Investigation of Physiological Activity of the Cerebral Cortex*, 1927, 1-430.

[4] C. L. Hull, The concept of the habit-family hierarchy and maze learning: I., *Psychol. Rev.*, 41, 1934, 33-54.

[5] H. M. Valentine, Visual perception in the white rat, *J. Comp. Psychol.*, 8, 1928, 369-375.

publications,[6] and other literature on conditioned responses in man or lower animals, reveals no explicit statement or data which would even lead with certainty to the conclusion that duplicate stimuli will produce an increment or a decrement in response, according to the phase of the conditioning process in which they be presented.[7]

From these considerations two major questions arise: (1) Can *duplicate* stimuli effect (on different occasions) both external inhibition and disinhibition of a given conditioned response when it is in an excitatory and an inhibitory phase respectively? If so, what will be the relative functional strengths of the inhibited and disinhibited responses? (2) Can the *same* stimulus *simultaneously* effect external inhibition of one conditioned response and disinhibition of another conditioned response? If so, what will be the relative functional strengths of the inhibited and disinhibited responses? The present investigation attacks the first question.

Apparatus and Procedure

Apparatus. The apparatus and technique used were essentially similar to those previously employed by Switzer[8] and by Bass and Hull[9] in conditioning electrodermal responses.[10] A Leeds & Northrup D'Arsonval galvanometer (No. 2239a) was connected in series with *S* by means of two dry brass electrodes the contacting surfaces of which had been coated with solder. The smaller electrode (22 mm. in diam.) was placed on the back of *S*'s hand and the larger (37 mm. in diam.) was placed on the palm. Both were held in position by a strap of adhesive tape. A light was reflected from the mirror of the galvanometer to a sanded celluloid screen at a distance of 65 cm., upon which the movement of light could manually be traced. The movements of the tracing pointer were reduced by a four to one pulley and recorded on stylograph paper moving at a constant speed of 10 cm. per min. A variable external resistance consisted of three potentiometers in series with a maximum rating of 650,000 ohms.

The source of the stimulus to be conditioned was a clear glass red Westinghouse 25-watt photographic bulb which measured 1.241 cp. at one meter. The 'extra' stimulus (used for external inhibition and for disinhibition) was a Hull tactual vibrator actuated by a Lionel type T alternating current transformer, and producing 120 double vibrations per sec.[11] It was fastened by adhesive tape to the middle of the dorsal surface of *S*'s left hand and the hand was maintained in prone position so that the core was always resting on *S*'s skin when no current was flowing.

[6] *Op. cit.*, and *Lectures on Conditioned Reflexes*, 1928, 1-414.

[7] Though such a statement is made by Bekhterev, no illustrative data are presented. Cf. V. M. Bekhterev, *General Principles of Human Reflexology*, 1933, 268.

[8] S. A. Switzer, Disinhibition of the conditioned galvanic skin response, *J. Genet. Psychol.*, 9, 1933, 77-100.

[9] M. J. Bass and C. L. Hull, The irradiation of a tactile conditioned reflex in man, *J. Comp. Psychol.*, 17, 1934, 47-65.

[10] The terminology recently suggested by Ruckmick is here used. *Cf.* C. A. Ruckmick. Terminology in re "psycho-galvanic reflex," *Psychol. Rev.*, 40, 1933, 97-98.

[11] *Cf.* Bass and Hull, *op. cit.*, p. 49.

For 10 *Ss* (Group I) the vibrator was activated by 8.9 v. and 2.2 amp. of current. For 10 other *Ss* (Group II) it was activated by 11.6 v. and 2.8 amp. which elicited approximately its maximal intensity. The unconditioned stimulus was a tetanizing faradic shock derived from the secondary of a Porter inductorium. The primary received a current of 4 v. and 2.7 amp. The shock was applied to the ball of the right foot by a brass electrode 10 mm. square. The diffuse electrode was a sheet of copper 7.5 by 4.5 cm., slightly curved in order to fit firmly against *S*'s right calf.

S lay on a cot and the foot electrode, adjustable in height, was fastened to the wall just above the cot. It was supported by a spring so that when the foot was withdrawn during the experiment a circuit was closed actuating a magnetic marker, thus recording all foot withdrawals in excess of about 2 mm. The head of *S* was enclosed at the back, top, and sides by a dull black cardboard hood, 75 cm. square, from the front of which two layers of black cambric cloth were dropped and allowed to rest over *S*'s chest. On the inside of the hood, on a level with the median plane of *S*'s head were two lamp sockets. One contained a 25 w. blue frosted bulb, measuring 0.1137 cp. at one meter, which formed a source of constant illumination. The other contained the source bulb of the stimulus to be conditioned. Its incidence was perceived as a slight change in color and intensity of light reflected from the roof of the hood.

A pair of 2000 ohm head-phones activated by 6-v. alternating current from a Lionel type T transformer served as a sound screen. Cotton was also placed in *S*'s ears. Stimulations were delivered automatically by a telechron-motor-driven switch revolving at a constant speed of 1 r.p.m. The duration of the light stimulus was 1.65 sec. Then 0.92 sec. after its incidence the shock was given. Its duration and rate were 0.94 sec. and 50 per sec. respectively. The tactual vibrator (extra stimulus) had a duration of 0.70 sec. All stimulations were recorded upon the stylographic paper by magnetic markers.

Procedure. After preliminary work the main experiment was set up in which 27 men, all undergraduate students, were used as the *Ss*. The data obtained from 7 of them were discarded for the following reasons: insufficient reaction to shock to produce foot withdrawal (2 *Ss*); insufficient evidence of conditioning (2 *Ss*); excessive spontaneous electro-dermal reactions rendering interpretation of records questionable (2 *Ss*); voluntary anticipatory foot withdrawal to evade shock (1 *S*). The remaining 20 *Ss* were divided into the 2 groups of 10 each. Every *S* was given the following written instructions.

> This is an experiment concerning the reactions of the human body to slight electrical shocks. Its success depends upon your full coöperation for about an hour and a half. During that time it is essential that you (1) *Have your eyes open at all times;* (2) relax your muscles as much as possible and make no unnecessary movements; (3) refrain from carrying on mental activity if possible (relax your mind as well as your body); (4) do not whisper or whistle to yourself; (5) speak only if necessary; and (6) please refrain from deep breathing or sighing.
>
> You will receive not less than 10 nor more than 30 slight shocks, and from time to time will see a faint increase in light or feel a vibration on the back of your left hand.

The instructions were emphasized by *E* as *S* was being adjusted in the apparatus. A shock of sufficient intensity to produce a slight foot withdrawal was then empirically determined, the head-phones placed in position, and the polygraph started. After a period, 3–5 min. in duration, allowed for the adaptation of *S* to the experimental situation, stimulations were begun.

The basic plan of experimentation was simple. It was first necessary to condition rather thoroughly the electro-dermal response to the red light by paired repetitions of the light and shock stimuli. After conditioning was effected and its functional strength measured by testing with the light alone, the vibrator was given as an extra stimulus shortly before presenting again the light without the shock. The after-effect of the vibratory stimulation, according to the principle of external inhibition, should inhibit the conditioned reaction to the light, resulting in a decrement in response. The amount of decrement would be the measure of the external inhibition. Following this the conditioned response was approximately extinguished by stimulating with the light many times without reënforcement by shock. Now, according to the principle of disinhibition (removal of extinctive inhibition), presentation of the vibrator before the light should result in an increment in response to the latter. The procedure of stimulation by means of which this was accomplished follows.

Summary of Procedure

Primary tests. Stimulus to be conditioned (red light); interval, approximately 38 sec.; extra stimulus (vibrator); interval of 19.94 sec.; stimulus to be conditioned; interval of 1 min.[12]

Conditioning process. Ten conditioning stimulations (paired red light-shock); conditioned stimulus (red light alone); ten conditioning stimulations; conditioned stimulus; five conditioning stimulations;[13] conditioned stimulus; interval 5 min.

Test series for external inhibition. Conditioned stimulus; interval approximately 38 sec.; extra stimulus; interval 19.94 sec.; conditioned stimulus (first test); interval 2 min.; conditioned stimulus; interval approximately 38 sec.; extra stimulus; interval 19.94 sec.; conditioned stimulus (second test); interval 2 min.; conditioned stimulus.

Extinction process. Conditioned stimulus (consecutive presentation);[14] interval approximately 38 sec.

Test series for disinhibition. Conditioned stimulus; interval approximately 38 sec.; extra stimulus; interval 19.94 sec.; conditioned stimulus (first test); interval 2 min.; conditioned stimulus; interval approximately 38 sec.; extra stimulus; interval 19.94 sec.; conditioned stimulus (second test); interval 2 min.; conditioned stimulus.[15]

The criterion for external inhibition was that the conditioned electro-dermal response to light following vibratory stimulation should be smaller in recorded

[12] As this test in Group I indicated that the vibratory stimulation might have a slight depressor effect upon the unconditioned response to light, a second light stimulation was presented 1 min. later in Group II (cf. Fig. 1).

[13] Although preliminary work indicated that the conditioned electrodermal response was invariably present after 8 paired stimulations, the response was not always sufficiently stable to undergo the protracted unreënforced presentations required. Therefore 25 conditioning stimulations were arbitrarily given to Group I. As a large number of unreënforced presentations of the conditioned stimulus proved necessary with this group before approximate extinction was obtained, the number of conditioning stimulations for Group II was reduced to 20. They were given at random intervals of never less than 1 min. and seldom more than 2 min.

[14] These stimulations were begun at intervals of 1 min. If the conditioned response showed no decrement after several presentations, the conditioned stimulus was presented at a rate increased to 2 or 3 per min. by means of a manually operated key. These unreënforced stimulations were continued until the response approached its primary amplitude.

[15] Since spontaneous recovery was evidenced in most of the *Ss* in Group I after the 2-min. intervals following the tests for disinhibition, a second extinction series was used for Group II.

amplitude than the mean of the immediately preceding and following responses to light alone. The criterion for disinhibition was that the response to light following vibratory stimulation should be greater in recorded amplitude than that of the approximately extinguished response on the preceding test. Since the response to light 2 min. after the test for disinhibition resulted more often in an apparent spontaneous recovery than in a reinstatement of extinctive inhibition, a more rigorous criterion for disinhibition was impossible.

Results and Discussion

Inhibition and disinhibition from duplicate stimuli. A general picture of the experimental results is presented in Fig. 1. It will be seen that the mean responses for each group of *S*s show both external inhibition and

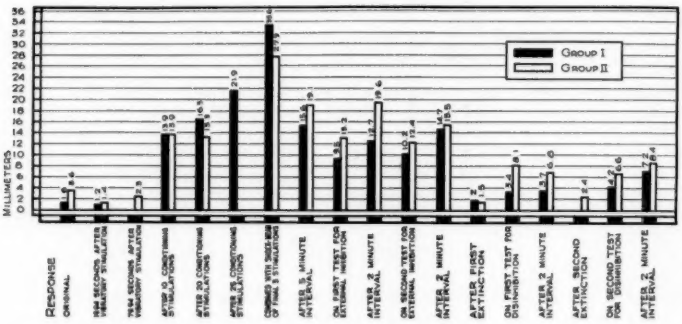

Fig. 1. Mean Galvanic Measurements, in Mm., of Electro-Dermal Responses to the Conditioned Stimulus Throughout the Experiment

disinhibition as would be expected from an application of Pavlov's principles. That *both external inhibition and disinhibition can result from duplicate extraneous stimulation is thus demonstrated.* That the result is, however, not invariable may also be learned by an inspection of Table I which reproduces the values from which determinations of inhibition and its removal were made. It will be seen that the after-effect of extraneous stimulation from the tactual vibrator caused a decrement in the conditioned response in 30 out of 40 tests and an increment in the relatively extinguished response in 29 out of 40 tests. In the other 21 tests the vibratory stimulation did not cause the predicted change in response. In 3 of these no change was noted, while in the other 18 a reversal effect was recorded, *i.e.* an increment in response resulted where a decrement was expected (10 tests) or a decrement in response occurred where an increment was expected (8 tests).

Relative strengths of inhibited and disinhibited responses. Table II shows

TABLE I

GALVANIC MEASUREMENTS FOR DETERMINATION OF EXTERNAL INHIBITION AND DISINHIBITION, MILLIMETERS

(C.R. = criterion conditioned response; 1st. = first test for inhibition; D = amount decrement; R.I. = amount reversal increment; 2nd. = second test for inhibition; E.R. = extinguished response; 1st.D. = first test for disinhibition; I = amount increment; R.D. = amount reversal decrement; 2nd.D. = second test for disinhibition; R.D. = amount reversal decrement; X. = functional crossing; E. = functional equalization.)

	S	Tests for external inhibition							Tests for disinhibition							Functional crossing	
		C.R.	1st.	D.	R.I.	C.R.	2nd.	D.	R.I.	E.R.	1st.D.	I.	R.D.	E.R.	2nd.D.	I.	R.D.	X.	E.
	1	11.5	8.0	3.5		9.0	10.0		1.0	0.5	2.5	2.0		8.0	1.0		7.0		
	2	13.0	12.0	1.0		16.5	11.0	5.5		1.0	4.0	3.0		6.0	11.0	5.0			E
	3	35.0	17.0	18.0		33.0	21.0	12.0		1.0	3.0	2.0		5.0	0.0		5.0		
	4	4.5	10.0		5.5	9.25	4.0	5.25		5.0	3.0		2.0	6.0	6.0	0.0		X	
Group I	5	17.5	3.0	14.5		4.0	0.0	4.0		3.5	3.0		0.5	1.0	6.0	5.0		X	E
	6	4.25	2.0	2.25		3.0	3.5		0.5	1.5	3.0	1.5		1.0	1.5	0.5		X	
	7	15.0	10.0	5.0		17.5	21.0		3.5	2.0	1.0		1.0	1.0	0.0		1.0		
	8	24.0	21.0	3.0		30.0	14.0	16.0		0.0	8.0	8.0		1.5	12.0	10.5			
	9	10.0	6.5	3.5		7.0	8.0		1.0	0.0	0.5	0.5		0.0	1.0	1.0			
	10	7.25	6.0	1.25		8.0	9.5		1.5	5.5	6.0	0.5		7.5	3.5		4.0		E
	Total	142.0	95.5	52.0	5.5	137.25	102.0	42.75	7.5	20.0	34.0	17.5	3.5	37.0	42.0	22.0	17.0		
	Mean	14.2	9.5	5.2	0.5	13.7	10.2	4.3	0.7	2.0	3.4	1.7	0.3	3.7	4.2	2.2	1.7		
	11	17.0	15.0	2.0		19.5	14.0	5.0		0.5	4.0	3.5		6.0	11.0	5.0			
	12	15.25	10.5	5.0		14.25	9.5	4.75		1.5	8.5	7.0		1.0	7.0	6.0			
	13	8.25	9.0		0.75	8.25	8.5		0.25	0.0	10.0	10.0		0.0	3.0	3.0		X	
	14	20.0	13.0	7.0		10.5	10.0	0.5		0.0	7.0	7.0		0.0	7.0	7.0			
Group II	15	29.0	24.0	5.0		24.5	13.0	11.5		3.0	1.0		2.0	0.0	4.0	4.0			
	16	11.5	12.0		0.5	9.0	7.0	2.0		0.0	1.5	1.5		0.0	1.0	1.0			
	17	20.5	1.0	19.5		13.75	1.0	12.75		0.0	0.5	0.5		1.0	12.0	11.0		X	
	18	37.5	17.0	20.5		36.0	20.0	16.0		0.0	0.0	0.0		0.5	3.5	3.0			
	19	26.5	26.0	0.5		33.0	40.0		7.0	10.0	46.0	36.0		16.0	18.0	2.0		X	
	20	8.0	4.5	3.5		6.75	1.0	5.75		0.0	3.0	3.0		0.0	0.0	0.0		X	
	Total	193.5	132.0	63.0	1.25	175.5	124.0	58.25	7.25	15.0	81.5	68.5	2.0	24.5	66.5	42.0	0.0		
	Mean	19.3	13.2	6.3	0.12	17.5	12.4	5.8	0.7	1.5	8.1	6.8	0.2	2.4	6.6	4.2	0.0		
	Mean I, II	16.8	11.4	5.7	0.3	15.6	11.3	5.0	0.7	1.75	5.7	4.2	0.2	3.1	5.4	3.2	0.8		

the ratios of the externally inhibited and disinhibited responses to the conditioned response.[16] It will be seen that the latter do not closely approach the former. Since 7 of the 8 previously mentioned reversal decrements occurred among the *Ss* of Group I who had no second extinction series and with whom extinction was not carried as near to the primary response value as was done with Group II, accurate comparison is only possible for the latter group. They are presented schematically in Fig. 2.

The externally inhibited response is seen to be approximately two-thirds of the amplitude of the conditioned response, while the disinhibited response attains approximately one-third of its original strength. It is apparent then that under the conditions of this experiment 'functional cross-

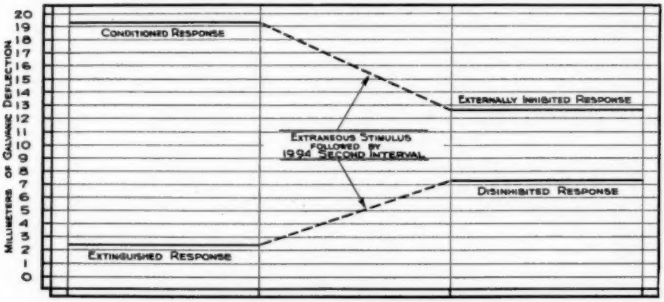

Fig. 2. Schematic Diagram Showing Mean Galvanic Measurements for Conditioned, Extinguished, Externally Inhibited, and Disinhibited Electro-Dermal Responses (Group II)

ing' would not be the average occurrence. An inspection of Table I shows that it did occur, however, in 4 of 20 tests, *i.e.* in 20% of them.[17]

[16] The mean of the responses immediately preceding and following the first test for external inhibition is regarded as the 'full value' conditioned response.

[17] It must be pointed out that, in the light of results recently obtained by the author and O. C. Irwin, these data cannot be considered decisive as they relate to the subject of functional crossing. These results indicate that skin resistance increases as relaxation and somnolence occur. The data reported in the present paper were reread and rather constant drifts in resistance were found in 18 of the 20 *Ss* during the extinction series, especially in its latter portion. Since the drifts were in the opposite direction from the majority of the recorded electro-dermal responses, it seems justifiable to regard them as indicative of increases in skin resistance. Furthermore, the experiments were usually conducted at night and many of the *Ss* reported great difficulty in remaining awake toward the close of the period. Wenger and Irwin have since shown that apparent skin resistance increases rapidly under such conditions of somnolence. (M. A. Wenger and O. C. Irwin, Variations in electrical resistance of the skin in new born infants and adults and their relation to muscular processes, *Univ. Iowa Stud., Child Welfare Monog. Ser.*, in press.)

Since the recorded response was probably an expressed bodily potential, the mag-

Autonomous disinhibition. In addition to the foregoing problems the data present certain other items of interest. One might be described as autonomous disinhibition during extinction. The average number of extinction trials necessary to reduce the conditioned response to approximately its original value was 21 for Group I and 30 for Group II. Additional unreenforced stimulations proved necessary in Group II before the second test for disinhibition was undertaken. The extinction series seems remarkably long in view of the fact that Switzer found only 19 mean trials necessary to produce extinction "below zero" after 31 mean conditioning stimulations.

TABLE II

RATIOS OF EXTERNALLY INHIBITED AND DISINHIBITED RESPONSES TO THE CONDITIONED RESPONSE

Ratio	Group I	Group II	Both
$\frac{\text{Mean response on first test for inhibition}}{\text{Mean conditioned response}}$	.67	.68	.68
$\frac{\text{Mean response on second test for inhibition}}{\text{Mean conditioned response}}$	.72	.64	.68
Combined ratios for inhibition	.70	.66	.68
$\frac{\text{Mean response on first test for disinhibition}}{\text{Mean conditioned response}}$	.24	.42	.33
$\frac{\text{Mean response on second test for disinhibition}}{\text{Mean conditioned response}}$	.30	.34	.32
Combined ratios for disinhibition	.27	.38	.32

It has already been stated that the *S*s of this experiment reported somnolence during the extinction series. Although they had been instructed to

nitude of which would be affected by changes in resistance, it is possible to infer that responses at the time of the tests for disinhibition were subject to an error which was constant in direction. There true magnitude was reduced. If this inference is tenable, it is possible that functional crossing occurred but was concealed by the technique of measurement. In connection with this question it is of interest to compare the obtained results with those reported by Switzer (*op. cit.*, 92) on disinhibition. Switzer has made available to the writer his computations for the ratio of his mean first two disinhibited responses to his mean first two conditioned responses. His obtained ratio was 0.59 with *S*s seated. The ratio found in the present study was 0.38, with *S*s reclining. Since Wenger and Irwin found apparent skin resistance to be much higher in reclining and relaxed *S*s than in *S*s who were seated, it seems likely that the discrepancy in the ratios is attributable to different levels of skin resistance resulting from the different positions of the *S*s. Since knowledge of the exact level of resistance at the time of each measurement was not regarded as essential when the experiments were conducted, additional research is needed to determine which of the two ratios is the most reliable.

remain as quiet as possible, their efforts to remain awake resulted in many slight movements and adjustments in posture and a tendency toward sporadic deep inspiration. Almost invariably an increment in the conditioned responses followed. The proprioceptive sensations arising from these movements and inspirations are believed to have resulted in autonomous disinhibition and their frequent occurrence to have been directly responsible for the prolonged extinction series. This interpretation suggests that at least part of what is usually termed extinctive inhibition may be explained in terms of decreased level of tonicity. Bechterev has already mentioned such a possibility.[18] Dodge also relates muscular tonicity to one possible kind of inhibition.[19]

Rapidity of spontaneous recovery. The above suggestion may possibly explain the rapid recovery of the extinguished responses immediately after disinhibition. Vibratory stimulation after extinction often produced a withdrawal of the hand. The startle effect, with or without withdrawal, was certainly greater muscularly than the previously mentioned autonomous responses, and it was usually followed by additional readjustments in position. If the assumption be accepted that a portion of the recorded extinction was caused by a decreased level of tonicity it is conceivable that the rapid apparent spontaneous recovery was due to persisting proprioceptive stimulation resulting in a new tonus level. Such a postulate would also explain a phenomenon described by Pavlov, who wrote that "in every case . . . it is possible to accelerate the restoration of an extinguished conditioned reflex. For this purpose it is only necessary to apply the unconditioned stimulus on which the conditioned reflex was built up, either singly, or together with the extinguished conditioned stimulus."[20] It is obvious that presentation of an adequate unconditioned stimulus to a resting organism would serve to interrupt a state of muscular relaxation.

Differential effect of two intensities of extraneous stimulation. It has already been stated that for the 10 *S*s of Group II the tactual vibrator was activated by a stronger current than was used for Group I. The change was made in the hope of obtaining greater inhibitory and disinhibitory effects than were found in the first group. The differential effect of the two intensities may be seen in Fig. 1 and in the following tabulation drawn from Table I.

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[18] *Op. cit.*, 270.

[19] Raymond Dodge, *Conditions and Consequences of Human Variability*, 1931, 1-162.

[20] Pavlov, *Conditioned Reflexes*, 1927, 59.

	External inhibition decrement (mm.)		Disinhibition increment (mm.)	
	1st test	2nd test	1st test	2nd test
Group II:Mean	6.3	5.8	6.8	4.2
Group I: Mean	5.2	4.3	1.7	2.2
Differential	1.1	1.5	5.1	2.0

It will be seen that the greater intensity of extraneous stimulation was more effective both as an external inhibitor and as a disinhibitor. The difference is particularly noticeable on the first test for disinhibition.

Infrequency of conditioned foot-withdrawals. Although one of the *Ss* in the preliminary experiments showed a stable anticipatory foot-withdrawal as well as a conditioned electro-dermal response, both of which demonstrated external inhibition, no such results were found in the main experiments. Only 7 anticipatory withdrawals were recorded by 5 of the 20 *Ss*. Some reports of voluntary inhibition of the "desire" to remove the foot were given, and the *Ss* invariably report a tensing of the muscles of the right leg in response to presentation of the conditioned stimulus.

Effect of vibrator upon original response to light. Fig. 1 shows that the mean unconditioned electro-dermal response to light 19.94 sec. after stimulation by the tactual vibrator is slightly smaller than it was on the test preceding vibratory stimulation. On another test made 1 min. later (Group II) the response was more nearly its original value. The results thus indicate that the tactual-vibratory stimulation may have had a slight inhibitory effect upon the response to light. There was obviously no irradiation effect, a finding which agrees with that of Switzer.[21]

SUMMARY

A study has been made to discover whether duplicate extraneous stimuli can effect both external inhibition of a conditioned response and disinhibition of the same response after experimental extinction; and, if so, to determine their relative functional strengths. Galvanic measurements of the conditioned electro-dermal response were employed. The response, instituted by an electrical shock applied to the sole of the right foot, was conditioned to a faint increase and change of hue in illumination by means of 20 to 25 paired light-shock stimulations. After conditioning, external inhibition was attempted by presentations of a tactual-vibratory stimulus 20 sec. before the conditioned stimulus. After experimental extinction the same stimulus was presented with the same time interval. Two groups of *Ss* were used, each composed of 10 men, all undergraduate students. Group II received a more intense extraneous stimulus than did Group I. The results are as follows.

[21] *Op. cit.*, 85.

(1) External inhibition and disinhibition were produced by duplicate stimuli.

(2) The effect of extraneous stimulation by the tactual vibrator was not invariable at either phase of the experiment. During the 40 tests for external inhibition reversal effects (increments in response) occurred 10 times; during the 40 tests for disinhibition reversal effects (decrements in response) occurred 8 times and in 3 cases there was no recordable effect.

(3) It has been pointed out that in the light of recent research the data concerning the functional strengths of the externally inhibited and disinhibited responses cannot be considered decisive.

(4) Autonomous disinhibition believed to be due to postural adjustments and transient interruptions of somnolence made necessary a protracted extinction series. A possible relationship between decreased tonus level and extinctive inhibition is noted.

(5) There is some indication that the greater of the two intensities of tactual vibration used produced a greater mean amount of external inhibition and disinhibition than did the weaker stimulus.

(6) Conditioned anticipatory foot-withdrawals were infrequent. Muscle tension of the right leg subsequent to presentation of the conditioned stimulus was invariably reported.

CUTANEOUS LOCALIZATION AMONG NORMALS AND SUBNORMALS

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In genetic studies of cutaneous localization Renshaw found that visual dominance is so much more pronounced among adults than among children that adults make more accurate localizing responses on a visual schema of the skin surface stimulated.[1] Children on the other hand, having less visual dominance, localize better when blindfolded and allowed to point to the actual skin surface stimulated. We proposed in the present investigation to compare the performance of subnormal and normal individuals who had reached a fair degree of physical maturity, in order to find out to what extent subnormals function like normals in showing visual dominance in skin orientation.[2]

In a previous experiment we made a comparison of tactual-kinesthetic and visual perceptions of extent among children and normal and subnormal adults, where the task was to reproduce an initial tactual-kinesthetic perception of extent by either tactual-kinesthetic or visual means.[3] We found that the adult sub-normals reacted in many respects more like the normal adults than like the children. For example, both adult groups showed greater visual dominance than did the younger group, in that they both exaggerated the visually reproduced estimates of initial tactual-kinesthetic perceptions of extent a good deal more than did the children. The performance of the subnormals was, however, decidedly inferior to that of the normal adults both in tactual-kinesthetic and in visual methods of response, and particularly in the latter. We felt it would be of interest to investigate further the similarities or dissimilarities of performance of subnormals and normals in a simpler task than that of estimating extents and in one less complicated by constant errors due to perceptual illusions.

METHOD AND PROCEDURE

Procedure. The flat surface of the volar forearm of the non-preferred arm midway between elbow and wrist was chosen as the area of stimulation. A cross-sectioned pattern 5 x 5 cm. (each cross-section 5 x 5 mm.) was printed on the skin surface. An area 3 x 3 cm. in the center of the pattern was taken as the stimulation area. The

* Accepted for publication October 10, 1935.

[1] S. Renshaw, The errors of cutaneous localization and the effect of practice on localizing movement in children and adults, *Ped. Sem.*, 38, 1930, 223-238; S. Renshaw and R. J. Wherry, Studies on cutaneous localization: III. The age of onset of ocular dominance, *ibid.*, 39, 1931, 493-496.

[2] Renshaw and Wherry found that visual dominance was very apparent at 14 yr. and became well established at 16 yr. *Op. cit.*, 494.

[3] The author, A comparison of tactual-kinesthetic and visual perceptions of extent among adults, children and subnormals, this JOURNAL, 48, 1936, 269-296.

stimulator was a brass stylus with hard rubber point (2 mm. in diam.) adjusted to give a constant pressure (930 g. per cm.^{2}). In the tactual-kinesthetic localizing method (TK) without direct visual control, we employed the procedure used by Renshaw (Weber's second method). *S* was blindfolded. He placed his non-preferred arm, volar side up, on a table, held a stylus (tip 1 mm. in diam.) in his preferred hand, and localized by aiming from a distance of 15 in. above the surface of the forearm. His initial aim without exploration of the skin was taken as his response. In the localizing method with visual control, Renshaw had his *S*s look at a visual schema of the arm or hand and make their localizing response by either pointing to the position in the diagram or calling out the position in the diagram (by naming the coördinates in a cross-sectioned pattern) corresponding in their estimation to the initial point stimulated. In the earlier work on tactual localization other visual methods had been employed, such as localizing on a model or on a photograph of the skin area.[4] We felt that in all of these experiments the task was predominantly one of identifying a drawing, model, or photograph of the skin surface with the actual area stimulated, and that this method required more complicated functioning (imaginative) than a method where the localizing could be made on the site of the initial tactual perception. Our intention was to have the visual control task as simple as possible, thus making it more comparable to the tactual-kinesthetic task without visual control. Hence in our visual localizing method (V), *S* held the stylus horizontally in his preferred hand, which rested on the table 4 in. from the other arm. After applying the stimulus *E* quickly lifted the black cloth hood that covered *S*'s eyes. *S* made his localizing response by looking at the cross-sectioned pattern on his arm and pointing with the stylus to a point as near his skin surface as possible without making contact.[5] *S* was required to hold the stylus in this position for a moment until *E* lowered the hood over his eyes and measured the error of his response. In both the TK and V methods, records were taken of the difference in millimeters between the point at which the stimulus had been applied and the point indicated in the localizing response. Throughout the experimenting the time of stimulation (0.5 sec.), speed of response (aiming and pointing movements), and position of both arms were kept as constant as possible.

Subjects. The normal group of *S*s consisted of 10 girls whose chronological ages (C.A.) ranged between 15 yr. 11 mo. to 16 yr. 11 mo., who were taking postgraduate work in skilled garment trades in an industrial high school. Their I.Q. on the Otis Self-Administering Examination, Higher Form B, ranged between 85 and 110. Their records on the academic work required in their two years of high school training had been good or excellent.

There were two groups of subnormal *S*s. One group consisted of 10 girls whose C.A. ranged between 16 yr. 0 mo. to 16 yr. 11 mo., who were taking training in vocational classes for subnormals. These girls had been given the Otis Self-Administering Examination, Intermediate Forms A and B, in two successive years and had

[4] V. Henri, La localisation des sensations tactiles, *Année psychol.*, 2, 1895, 168-192; W. B. Pillsbury, Some questions of cutaneous sensibility, this JOURNAL, 7, 1895-96, 42-57.

[5] As the tip of *E*'s stylus was 2 mm. in diam. no identifying mark was left on the skin surface after stimulation. The pressure of the stylus, however, was sufficient to give a clear tactual perception.

obtained I.Q. on both tests between 50 and 64. On the Pintner Non-Language Test their mental ages (M.A.) ranged from 8 to 12 yr. with one exception (one girl had an M.A. of 15 yr.).

The second subnormal group was composed of *Ss* from Letchworth Village (a New York state institution for mental defectives), and was selected on the basis of special visual ability. In a study of the Goodenough Drawing Test for the Measurement of Intelligence among subnormals, Kinder found a few *Ss* (about 15 out of 250) who scored exceedingly high on this test as compared with their scores on the Stanford-Binet intelligence test, having an M.A. rating at least 3 years higher on the former than on the latter test.[6] Now Goodenough obtained a correlation between her test and the Stanford-Binet of $+ 0.74 \pm .016$.[7] Hence it is suggested that this specially selected group of *Ss* had a good deal more proficiency in organization of visual space perceptions and in eye-hand coördinations than in the comprehension and handling of verbal material.[8] We were interested in finding out to what extent these same individuals would show visual dominance in our experiment on tactual localization. We chose from among these *Ss* those whose C.A. was 14 yr. or more. The group consisted of 6 white boys with C.A. between 14 yr. 4 mo. and 16 yr. 3 mo.; and 5 negroes, 3 girls and 2 boys, with C.A. between 14 yr. 7 mo. and 15 yr. 4 mo., with the exception of one boy whose age was 21 yr. The white boys had I.Q. on the Stanford-Binet ranging from 53 to 69 and scores in points on the Goodenough drawing test between 32 and 45 (M.A. equivalents 11 to over 13 yr.). The negro group had I.Q. on the same test between 47 and 74.5 and scores on the Goodenough test between 41 and 49 (M.A. equivalents all over 13 yr.). For the sake of group homogeneity the scores for the white boys and negroes are kept separate in presenting our results.

Each *S* came for 7 experimental periods, with the exception of one normal *S* who came for only 4 periods. No *S* had more than 3 periods a week, with an interval of at least 24 hr. between periods. In each period 10 preliminary localizations were made, 5 by the TK and V methods respectively, followed by 40 recorded responses. There were 20 recorded responses by each method, the methods alternating after each 5 localizations. A rest of 5 min. was given half way through the experimenting in a given period. The total number of localizations by both the TK and V methods were 2680 for the normal girls, 2800 for the subnormal girls, 1680 for the subnormal boys and 1400 for the subnormal negroes. The *Ss* were told that this was an experiment to see how well boys and girls could do in a task when they were either allowed or not allowed to look at what they were doing. The *Ss* were encouraged to do as well as they could at each experimental period. The experiment was carried on during the winter and spring of 1935 at the Manhattan Industrial High School for Girls and in the Research Department of Letchworth Village.[9]

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[6] E. F. Kinder, An approach to problems in the field of psychology at a state school for mental defectives, *Proc. Amer. Assoc. Ment. Deficiency*, 59, 1935, 44-57.

[7] F. L. Goodenough, *Measurement of Intelligence by Drawings*, 1926, 51.

[8] On the Stanford-Binet test the *Ss* did particularly well on the 'copying designs' test at the 10-yr. level.

[9] Thanks are due Miss Florence Marshall, Principal of the Manhattan Industrial High School, and Dr. C. Little and the Research Staff of Letchworth Village for making this study possible.

RESULTS

Scoring was in terms of magnitude of errors in mm., and averages for these scores were computed for each group as a whole and for the individual *Ss* in each group (Table I. The normal group and the special subnormal groups, boys and negroes, did better by the V than by the TK method (the critical ratios between the actual differences in average scores and the P.E.$_{diff.}$ being respectively 1.5, 3.0, and 3.7 times those required for complete statistical reliability). The subnormal girls, on the other hand, did very much better by the TK than by the V method (the critical ratio being 4.2 times greater than that required for complete statistical reliability). Thus, in considering the performance of the groups as a whole, we see that the normal group and the special subnormal groups reacted as did the normal adults in Renshaw's experiment in localizing more accurately by a visual method, whereas the subnormal girls reacted as did the children in Renshaw's experiment and localized more accurately when aiming without direct visual control.

Relative performance. The relative performance of the groups by the TK and V methods respectively was next compared. By the TK method the normal girls and subnormal boys did better than the subnormal girls and subnormal negroes, although only in the case of the normal girls was the score fairly significantly superior to that of the two poorer groups (actual diff. in av. score between normal and subnormal girls, 0.644, P.E.$_{diff.}$ 0.181, crit. ratio 3.55; actual diff. between normal girls and subnormal negroes 0.9, P.E.$_{diff.}$ 0.214, crit. ratio 4.21). Renshaw claimed that children do better than adults by the TK method since they have less interference from visual mechanisms. In this respect none of our subnormal groups reacted like children but rather like less skilled adults.

By the V method the normal girls did very much better than the subnormal girls (actual diff. in av. scores 5.048, P.E.$_{diff.}$ 0.187, crit. ratio 26.99). In fact the critical ratio is 8.7 times that required for complete statistical reliability of difference. The special subnormal groups, boys and negroes, also did very much better than the subnormal girls by this method of direct visual control. The actual difference in score between the subnormal girls and boys is 6.328, P.E.$_{diff.}$ 0.192, crit. ratio 32.95, and the actual difference in score between the subnormal girls and the negroes is 6.636, P.E.$_{diff.}$ 0.201, crit. ratio 33.01. What is more, the performance of the subnormal boys and negroes by the V method was significantly superior to that of the normal girls. The actual diff. in av. score between the normal girls and subnormal boys is 1.28, P.E.$_{diff.}$ 0.179, crit. ratio 7.15, and the corresponding difference between the normal girls and subnormal negroes is 1.588, P.E.$_{diff.}$ 0.189, crit. ratio 8.4. Thus the special subnormal groups showed greater visual dominance, while the unselected subnormal group showed less visual dominance, than the normal group.

Individual scores. When we consider the individual scores for the *Ss* in each group (Table I), we notice that in the normal group 8 out of 10 *Ss* did better

TABLE I

AVERAGE ERRORS (IN MM.) AND MEASURES OF RELIABILITY OF DIFFERENCES BY TK AND V METHODS OF LOCALIZING OF INDIVIDUAL AND TOTAL SCORES FOR NORMAL AND SUBNORMAL GROUPS

	S	No. trials	Tac-kin. Av. score	Tac-kin. P.E.	Vis. Av. score	Vis. P.E.	Diff.	$P.E._{diff.}$	Crit. ratio
Normal girls (N=10)	*Bo*	140	13.06	3.77	13.40	4.29	0.34	.484	0.70
	Co	80	16.45	4.60	9.70	3.35	6.75	.637	10.59
	Gu	140	12.48	3.31	9.92	3.16	2.57	.386	6.65
	Ja	140	12.23	3.95	13.86	4.22	1.63	.488	3.33
	LaS	140	14.28	4.86	9.60	2.94	4.68	.479	9.77
	Ma	140	16.54	4.80	14.37	3.80	2.17	.516	3.21
	Ro	140	14.26	4.60	19.52	3.55	5.26	.491	10.71
	Sca	140	13.63	4.03	12.46	3.79	1.17	.466	2.52
	St	140	14.68	3.90	12.54	2.94	2.14	.412	5.19
	Sto	140	10.0	3.47	10.00	3.61	0	.498	0
	Group	1340	13.64	4.32	12.66	4.11	.98	.162	6.07
Subnormal girls (N=10)	*Sa*	140	15.88	4.22	17.34	4.90	1.46	.546	2.67
	LaR	140	15.66	5.44	20.34	5.48	4.69	.655	6.15
	Co	140	15.43	5.56	18.40	5.06	2.97	.637	4.65
	Ho	140	12.23	3.95	16.83	4.64	4.60	.515	8.93
	Ot	140	11.60	2.75	12.20	3.19	.60	.356	1.68
	Be	140	14.54	4.46	19.60	5.64	5.06	.608	8.31
	Va	140	18.28	5.55	19.68	4.89	1.40	.624	2.24
	Pe	140	14.17	4.49	15.32	3.92	1.12	.503	2.23
	Sch	140	12.52	5.01	14.06	3.81	1.54	.532	2.89
	Yv	140	16.48	5.75	24.26	5.45	7.77	.669	11.60
	Group	1400	14.29	5.01	17.71	5.29	3.42	.194	17.62
Subnormal boys (N=6)	*Bu*	140	13.27	4.27	7.94	2.55	5.33	.42	12.68
	Ca	140	13.29	3.86	11.72	3.50	1.57	.439	3.58
	Ge	140	17.40	5.24	12.83	3.90	4.57	.552	8.28
	Ron	140	10.77	3.40	11.63	4.01	.86	.545	1.57
	Sch	140	14.09	4.39	12.46	3.83	1.64	.491	3.33
	Sk	140	14.28	4.15	11.31	3.54	2.97	.461	6.44
	Group	840	13.93	4.43	11.38	3.79	2.55	.201	12.67
Subnormal negroes (N=5)	*Ev*	140	15.40	5.46	12.20	3.52	3.20	.549	5.82
	Is	140	14.14	4.47	13.37	3.78	.77	.494	1.56
	Do	140	12.28	4.07	9.54	3.49	2.74	.453	6.05
	Gr	140	16.09	4.86	12.43	3.99	3.65	.531	6.89
	Ho	140	12.74	3.66	7.77	2.72	4.97	.386	12.88
	Group	700	14.55	4.76	11.08	3.82	3.47	.229	15.16

by the V than by the TK method; but among these only 6 did decidedly better (for the 2 others, *Ma* and *Sca*, the chances for the V scores excelling the TK scores are only 68% and 95% respectively). For one *S*, *Sto*, the scores are the same by both methods, and for another *S*, *Ro*, the score by the TK method is decidedly superior.[10] Among the subnormal girls, although all 10 performed better

[10] This *S* (*Ro*) had an I.Q. of 89, which was next to the lowest score in the normal group. Another *S* (*Co*), with the lowest I.Q. in the group (85), however, made among the best records by the V method, her record being a good deal superior to that of *Bo*, who had the highest I.Q. in the group (110).

by the TK than by the V method, in the case of only half of them do the differences in av. scores reach complete statistical reliability. The individual scores in the subnormal boy and negro groups reveal that 9 out of 11 *Ss* (82%) did reliably better (to the extent of 99% reliability of difference) by the V than by the TK method. The other 2 *Ss*, *Ron* in the boys' group, and *Is* in the negro group, also did better by the V method but not reliably so (only 85% reliability of difference between the TK and V score in each case).

From these results we see that in each group there were individual differences in accuracy of localization with and without direct visual control. Nevertheless, with the exception of one *S* (*Ro*) in the normal group, there is a fairly consistent trend in each group in favor of the TK method for the subnormal girls and in favor of the V method for the normal girls and for the subnormal boys and negroes. Undoubtedly, by repeating the experiment among a greater number of normals and subnormals and over a wider range of intellectual development (including superior adults) further individual differences would be revealed. Renshaw did not stress individual differences, but in the earlier work in this field of investigation, Henri stated that individual *Ss* favor either the TK or V method (identifying on a model) depending on the use made of visual imagery.[11]

Practice effect. As there has been a good deal of discussion and disagreement concerning the practice effect in experiments on localization, we computed the scores for each successive experimental period (Table II).[12] The normal girls did not show practice effects in either the TK or V methods, but performed fairly comparably throughout the successive periods. All the subnormal groups improved somewhat by both methods. In comparing the average scores for the first and last periods for each group we find the following differences.

Ss	Method	Diff. between first and last trials	P.E.$_{diff.}$	Crit. ratio
Normal girls	TK	−0.388	.437	0.88
	V	−0.444	.457	0.97
Subnormal girls	TK	+3.56	.494	7.21
	V	+2.58	.503	5.12
Subnormal boys	TK	+2.268	.598	3.79
	V	+1.536	.503	3.05
Subnormal negroes	TK	+2.36	.745	3.16
	V	+2.19	.561	3.90

The normal girls show no reliable difference in average scores by either the TK or V methods between the first and last (the 7th) periods. The three subnormal groups show improvement in the last period by both methods, although only in the

[11] Henri, *op. cit.*, 174. Other investigators also referred to individual differences in the use made of visual imagery in tactual space perceptions of the skin, cf. Pillsbury, *op. cit.* 46-47; M. F. Washburn, Über den Einfluss der Gesichtassociationen auf die Raumwahrnehmungen der Haut, *Phil. Stud.*, 11, 1895, 190-225.

[12] Practice effects are reported in the study of Renshaw (*op. cit.*, 227), but no practice effects are reported in the following studies: M. Ponzo, Ricerche e considerazioni intorno all'influenza dell'esercizio rappresentazioni spaciali cutanee, *Riv. di psicol.*, 8, 1912, 390-407; S. I. Franz, Accuracy of localization of touch stimuli on different bodily segments, *Psychol. Rev.*, 20, 1913, 107-128.

case of the subnormal girls is the difference in average score entirely reliable. In spite of their improvement, the subnormal girls still performed by the V method in the 7th period in a manner significantly inferior to that of the normal girls (actual diff. in av. score 2.836, P.E.$_{diff.}$ 0.465, crit. ratio 6.09).

Discussion

We shall first consider the performance of the normal and subnormal girls in cutaneous localization before taking up a discussion of the behavior of the specially selected subnormal groups. Now we have shown in our

TABLE II

Average Errors (in mm.) for Each Experimental Period by TK and V Methods of Localizing for Normal and Subnormal Groups

Period	Group							
	Normal girls		Subnormal girls		Subnormal boys		Subnormal negroes	
	TK	V	TK	V	TK	V	TK	V
1	13.50± 4.20	12.80± 4.40	16.58± 5.33	18.66± 5.49	15.60± 5.24	11.97± 4.03	16.56± 5.73	12.56± 4.12
2	13.88± 4.25	12.78± 4.14	15.06± 5.37	17.62± 4.43	14.20± 4.57	11.83± 3.70	14.28± 4.95	11.12± 3.54
3	13.20± 4.50	12.36± 3.86	14.36± 5.08	18.28± 6.21	15.07± 4.59	10.93± 3.26	13.68± 3.90	10.76± 3.55
4	13.10± 3.95	12.34± 3.98	12.92± 4.45	17.50± 4.97	13.73± 4.23	10.93± 3.62	13.92± 3.92	10.00± 3.28
5	14.44± 4.61	12.16± 4.03	14.80± 3.06	18.46± 5.10	12.63± 4.33	11.67± 3.68	14.56± 4.41	11.16± 3.86
6	13.62± 4.18	13.78± 3.86	13.32± 4.72	16.94± 4.91	12.37± 3.65	12.00± 4.10	14.28± 4.61	11.48± 4.12
7	13.89± 4.57	13.24± 4.53	13.02± 4.55	16.08± 4.55	13.33± 3.95	10.43± 3.77	14.20± 4.77	10.36± 3.82

previous investigation on perceptions of extent that both normals and subnormals performed less accurately by a V than by a TK method.[13] They both made constant errors with the V method by exaggerating the length of their visual estimates of previously perceived tactual-kinesthetic perceptions of extent, due perhaps to the fact that they were influenced by perceptual illusions (space-time illusions). We suggested that the reason for the greater predominance of the illusions among both groups of adults, normal and subnormal, than among the group of normal children was due to visual dominance in the former which might aid in a task that, like cutaneous localization, did not arouse space-time illusions, but would interfere with another type of task such as making visual reproductions of tactual-kinesthetic perceptions of extent.[14] We can understand, therefore, the difference in accuracy of performance of the normal girls by the V

[13] The author, *op. cit.*, 287.

[14] There are always some constant errors in cutaneous localization. We computed the number of errors made toward the elbow, toward the wrist, distally and proximally by, the TK and V methods for all our *Ss*. By the TK method all groups, except the subnormal boys, made more errors in the ulnar than in the radial direction. By the V method all of the groups with exception made more errors in the ulnar direction. Only the 2 groups of girls, subnormal and normal, showed constant errors in the horizontal directions; both made more errors distally than proximally by both the TK and V methods. In no group, however, was the number of constant errors particularly great. For a discussion of sources of constant errors in cutaneous localization see S. Hulin, The effect on tactual localization of movement during stimulation, *J. Exper. Psychol.*, 18, 1935, 97-105.

methods in the tasks of skin localization and perception of extent. We have yet to explain, however, the behavior of the subnormal girls in our present experiment. Why did they perform more like children in a task calling for orientation on the skin surface than in the seemingly more difficult task of estimating perceived extents? Why did not their visual dominance help them in making cutaneous localizations by the V method as it did the normal girls?

Now we must remember that the performance of the subnormal girls by the V method in the estimations of perceived extents was decidedly inferior to that of the normal girls. It is not surprising, therefore, that their performance by the V method in cutaneous localization was also decidedly inferior to that of the normal girls. We suggest that, even though the total perceptual experiences of subnormals (those who do not show special visual ability) may be more permeated with and influenced by visual aspects than are the perceptual experiences of younger normal children, the integration of these perceptions, *i.e.* the shift from one dominant perceptual field (tactual-kinesthetic) to another (visual) is harder for them than for normal adults. In other words, the visual mechanisms of the subnormals have not become sufficiently dominant to take precedence over or interfere with their tactual-kinesthetic mechanisms in the task of making body orientations. Subnormals still do better when they localize through tactual-kinesthetic means, even though it may well be that visual space perceptions play a greater rôle in their total experiencing of the spatial world around them than in the experiences of younger children. As has been seen, 50% of our subnormal *S*s did not do better by the TK than by the V method to the point of having the difference in average scores reach complete statistical reliability. What is more, when we considered the individual performance of our normal *S*s, we found that some individuals did not show any marked superiority when functioning by the visual method (one *S* doing decidedly poorly by this method). It would seem, therefore, that in order to understand thoroughly the perceptual functioning of both subnormals and normals in cutaneous localization, individual differences in the organization of the qualitative aspects of space perception need to be taken more carefully into account.

An attempt to do this, at least among subnormals, was made when we chose for *S*s in this experiment 2 groups of subnormals highly selected for their special ability on the Goodenough Drawing Test. The *S*s in these groups, boys and negroes, reacted not like the subnormal girls but like the normal girls, and showed a decided superiority of performance by the V rather than by the TK method of localization.

Now two or more questions may well be raised concerning the selection of *Ss* in these specially selected subnormal groups. In the first place, the majority of the *Ss* were boys (8 out of 11), while all of the *Ss* in the other 2 groups, normal and subnormal, were girls. The question is whether or not a sex difference accounted for the superior performance of the boys and negroes. By the TK method of localizing, however, the boys and negroes did not do reliably better than the subnormal girls, nor was their performance as good as that of the normal girls, so that the sex difference refers only to their achievement by the V method. It is true that Renshaw employed only male *Ss* in his investigation of visual dominance at different age levels. In all the literature on cutaneous localization, however, where both sexes have been used as *Ss*, there is no reference to the problem of sex differences in methods of localizing. Nor does it seem likely that there should be any sex differences in the organization and integration of perceptions in the various sense-modalities. Furthermore, we computed the scores separately for the 3 girls in the negro subnormal group and found their average scores and measures of reliability of difference to be as follows:

Method	No. trials	Av. score	Diff.	$P.E._{diff.}$	Crit. ratio
TK	420	14.58 ± 4.077	2.876	.277	10.38
V	420	11.704 ± 4.012			

The results show that these 3 girls made a decidedly superior record by the V than by the TK method (the crit. ratio being 2.5 times that required for complete statistical reliability of difference). In addition, if the V score for these girls is compared with the V score for the group of normal girls, we find a reliable difference in average scores in favor of the 3 negro girls (actual difference in av. score 0.948, $P.E._{diff.}$ 0.127, crit. ratio 7.46).

A more serious criticism of our selection of *Ss* lies in the matter of their classification as subnormal. In the first place, the I.Q. of the subnormal boys and negroes (Stanford-Binet 62.23, $\pm$ 6.43) were a little higher than those of the subnormal girls (Otis 57.5, $\pm$ 2.6). In the second place, the boys and negroes were selected because of their very superior performance on a non-language test of intelligence (Goodenough drawing test), while the subnormal girls with the exception of one *S* did quite poorly on a non-language test (Pintner). Thirdly, the fact that 5 of the 11 *Ss* in the specially selected group were negroes has to be taken into account. As negroes are quite likely to do badly on verbal tests, particularly on vocabulary tests, it is a question whether they can be considered of retarded mentality. But the 5 negroes in this experiment had lived in urban communities in the north for over 5 years (4 of them born in the north),

a factor which, according to Klineberg, makes their chances fairly good for obtaining scores on intelligence tests approximating those of the total population tested.[15]

In order to compensate for the possible inferior environment of negro life and, in addition, life in an institution for mental defectives, let us assume the average I.Q. for the boys and negroes in our group to be 10 points higher than it actually was on the Stanford-Binet test, making it 72.23. Even so we still have a good deal of difference between the intelligence level of this group and that of the normal girls (I.Q. on the Otis 95.7), a difference of 23 points in average score and with no overlapping of scores for the two groups.

Conclusion

Let us now return to our original problem of whether subnormals, who have reached approximate physical maturity, react more like normal adults or more like normal younger children in showing a preferred method of making body orientations. From the results of our experiment we can say that subnormals who ranked in the moron level of general intelligence and who did not show any marked ability in manual coördinations of visual space perception, performed more like children than like adults in that they did better by a tactual-kinesthetic method of localization than by a method guided by direct visual perception of the skin surface. Within each group, however, both subnormal and normal, there were some individuals who showed little preference for either method of localization. Furthermore, a group of subnormals, some of them above the moron level but decidedly not up to average in general intellectual development, who showed superior ability in the handling of visual space perceptions, reacted not at all like the other group of subnormals but like the group of normals in having decided visual dominance in making cutaneous localizations. In fact, their visual dominance was developed to the extent that their performance by the direct visual control method was superior to that of the normals. It remains for further investigation to determine how far down in the scale of mental retardation, individuals can be found who show visual dominance in ability to make body orientations.

[15] O. Klineberg, *Negro Intelligence and Selective Migration*, 1935, 1-66.

AN EXPERIMENTAL STUDY OF PERSONALITY, PHYSIQUE, AND THE ACID-BASE EQUILIBRIUM OF THE BLOOD

By J. A. HAMILTON and N. W. SHOCK, University of California

Several investigators have concerned themselves with the study of relationships between traits of personality, and alleged indices of the acid-base balance of the body. Working with Twitmyer's stammerers, Starr found that excitable *Ss* had a high salivary pH, while those designated by Twitmyer as 'sub-breathers,' had a low pH (acidic).[1] The sub-breathers, when given breathing exercises, responded by an improvement in speech and an increase in salivary pH. Rich found that excitability ratings correlated with salivary pH to the extent of +0.25-+0.45, indicating that excitable *Ss* tended to secrete alkaline saliva.[2] Ludlom reports an excited patient who was alkaline, and a confused patient with an acid diathesis.[3]

The theories of Kretschmer with regard to the association between certain 'types' of physique and mental disease have stimulated a considerable amount of quantitative research on the relationships between 'traits' and

* Accepted for publication December 20, 1934. From the Laboratory of Psychology and the Institute of Child Welfare, University of California.

[1] H. E. Starr, The hydrogen ion concentration of mixed saliva as an index of fatigue, etc., this JOURNAL, 33, 1923, 415-417; Psychobiochemistry, *Studies in Honor of Lightner Witmer* (R. A. Brotemarkle, ed.,) 1931, 155-167.

[2] G. J. Rich, A biochemical approach to the study of personality, *J. Abnorm. & Soc. Psychol.*, 23, 1928, 158-175.

[3] S. D. Ludlom, Physiologic psychiatry, *Med. Clin. of N. Amer.*, 2, 1918, 895. Cf. G. L. Freeman, *Introduction to Physiological Psychology*, 1934, 561. Summarizing Ludlom's paper, Freeman remarks; "he reports that the 'excited' type of insanity (circular?) gave an alkaline salivary reaction, together with alternating acid and alkaline condition of urine, perspiration, and feces, while the 'confused' type of insanity (schizophrenia?) gave a general acid reaction." It is particularly important to note that Ludlom was giving a preliminary report on but two patients and made no generalizations from his meager results.

The writers have unpublished data bearing on acid-base types in insanity. From one to five determinations of the acid-base equilibrium of the blood were made on each of 28 patients at the Agnews State Hospital. Determinations were made at one-week intervals. Diagnoses were obtained from the medical records of the hospital. The means of blood variables did not differ materially from those of our normal sample (cf. infra). The variability and dispersion of both manic-depressive and schizophrenic groups, however, were considerably greater. It is possible that, had the patients been classified on the basis of their behavior at the time of examination, group-differences might well have appeared. Unfortunately ratings of this type were not made for all the patients. The case of two schizophrenic females, upon whose behavior observations were available, are worth noting. Both had low serum bicarbonate when quiet and high bicarbonate when disturbed or elated. Three other patients showed equally wide fluctuations in behavior without marked acid-base changes.

physique.[4] Wertheimer and Hesketh devised an index of body-build based on trunk-volume and leg-length which seemed to be diagnostic of Kretschmer's classes of physique.[5] They found a tendency for the pyknic habitus to be associated with manic-depressive insanity and for the asthenic, athletic, and dysplastic types to be associated with schizophrenia. Mohr and Gundlach, examining prisoners, developed an index based upon three circumferences, weight and height.[6] They found pyknics less intelligent and specifically characterised in personality.

The Experiments

The development by Shock and Hastings of a micro-method for the determination of the acid-base balance, requiring but 1/10 cc. of blood for analysis, made it possible to use a relatively large number of *Ss*.[7] About 1/2 cc. of capillary blood was drawn, under oil, from the fingers of 137 male freshmen and sophomores at the University of California. Laboratory determinations of percentage of red cells, pH of the serum, and total carbon-dioxide content were made on three or four 1/10 cc. samples of this blood.[8] From these data it was possible to calculate the carbon-dioxide tension and total bicarbonate of the serum, using a nomogram for the solution of the Henderson-Hasselbalch equation.[9]

Several measures of personality were used. The Northwestern University test of

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[4] E. Kretschmer, *Körperbau und Charakter*, 1926 (6th ed.), 1-214.

[5] F. Wertheimer and F. E. Hesketh, A minimum scheme for the study of the morphological constitution in psychiatry with remarks on anthropometric technique, *Arch. Neur. & Psychiat.*, 17, 1925, 93; The significance of the physical constitution in mental disease, *Medicine*, 5, 1926, 375-467. The Wertheimer and Hesketh index is: leg-length cubed, times ten, divided by the product of trunk-length, chest-width and chest-depth.

[6] G. J. Mohr and R. H. Gundlach, The relation between physique and performance, *J. Exper. Psychol.*, 10, 1927, 117-157. The Mohr and Gundlach index is: chest plus abdomen plus hip plus weight, divided by height.

[7] N. W. Shock and A. B. Hastings, Studies of the acid-base balance of the blood: I. A micro-technique for the determination of the acid-base balance of the blood. *J. Biol. Chem.*, 104, 1934, 565-573; III. Variation in the acid-base balance of the blood in normal individuals, *ibid.*, 585-600.

[8] The writers wish to acknowledge the excellent technical assistance of Mrs. Olga Nave.

[9] A. B. Hastings and N. W. Shock, Studies of the acid-base balance of the blood: II. A nomogram for the calculation of the acid-base data for blood, *J. Biol. Chem.*, 104, 1934, 575-584; IV. Characterization and interpretation of displacement of the acid base balance, *ibid.*, 112, 1935, 239-261.

The Henderson-Hasselbalch equation is:

$$pH_s = pK_s' + \log [BHCO_3]_s/[H_2CO_3]$$

where:

pH_s = pH of serum

pK_s' = apparent first dissociation constant of carbonic acid in blood serum

$[BHCO_3]_s$ = concentration of bicarbonate in milli mols per liter of serum

$[H_2CO_3]_s$ = concentration of carbonic acid in milli mols per liter of serum

Introversion-Extraversion, developed by Gilliland, was administered.[10] The *Ss* were also given a shortened form of the Thurstone Neurotic Inventory, consisting of 160 items. These items comprise all of the groups in R. C. Tryon's unpublished classification of the neurotic inventory, with the exception of groups concerned with family relationships and personal health. All of the items designated as most diagnostic by Thurstone and Thurstone,[11] and Ellickson[12] were used. The experimenters rated most of the *Ss* on a seven-point scale of ease of adjustment to the standard situation of body-measurement in gymnasium-shorts.[13] The best-adjusted individuals were given a score of seven. All *Ss* were asked to rate themselves on a seven-point scale of introversion-extraversion. The self-ratings were made on the basis of a definition of introversion-extraversion, which was given the *Ss* on a mimeographed form to take home, and upon which they were requested to deliberate.[14] Conditions of routine and rapport with *Ss* made it necessary for them to make these self-ratings after they had been informed of their Northwestern score, and therefore the ratings are of questionable validity.

Physical measurements were taken with a Martin Anthropometer and steel tape, according to the technique described by Wertheimer and Hesketh (as above), Martin, and Hrdlička.[15] Measurements included height, weight, transverse and sagital chest-diameters, trunk-height, chest, abdomen and hip circumferences, and leg-length. Morphological indices calculated were the height-weight ratio, the Mohr and Gundlach index, and the Wertheimer and Hesketh index.

Results

Table I presents the intercorrelations between the various vital, personal, and morphological factors.[16] The Ns of the factors correlated varied from 112 to 137. While most of the correlations are statistically unreliable, when taken as a whole they show a surprising consistency. In this section we will confine our discussion to correlations of ± 0.15 and over.

Factors of personality measured by tests and ratings. The most curious of our correlations is that of $+0.17$ between the items taken from the Neurotic Inventory

[10] A. R. Gilliland and J. J. B. Morgan, An objective measure of introversion-extroversion, *J. Abnorm. & Soc. Psychol.*, 26, 1931, 296-304.

[11] L. L. Thurstone and T. G. Thurstone, A neurotic inventory, *J. Soc. Psychol., 1,* 1930, 3-30.

[12] Unpublished data on 750 University of Chicago freshmen.

[13] Criteria for 'ease of adjustment' were coöperativeness, interest, and conversation; while 'difficult adjustment' was judged in terms of reticence, shyness, lack of coöperation, and such physical symptoms as excessive perspiration and tremor.

[14] This definition read as follows; "The typical extravert is the sort of individual whose interests and energies are devoted to activities in the world about him. He enjoys social contacts and is 'a good mixer.' Any emotions which he experiences are freely expressed in facial movement and gesture. The introvert, on the other hand, is interested and absorbed in himself. He may experience strong emotions but rarely gives any outward expression of them. He enjoys being alone or with a few friends. He is interested in working with concrete objects rather than with people."

[15] Rudolph Martin, *Richtlinien für Körpermessung,* 1924, 19-30; A. Hrdlička, *Anthropometry,* 1920, 52-87.

[16] A preliminary report of these results, made at the meeting of the Western Psychological Association at Berkeley, in June 1934, is summarized in the *Psychol. Bull.,* 31, 1934, 584-585.

(Thurstone) and the Northwestern test. A tentative comparison of the Thurstone and Northwestern questionnaires indicates that many of the 'introvert' items are similar to Thurstone's 'neurotic' terms; while many of the 'extrovert' characteristics seem to be the opposite of Thurstone's 'neurotic' items. On *a priori* grounds one might thus expect a negative correlation between Thurstone and Northwestern scores, since a high Thurstone score indicates extraversion. When the actual construction of the questionnaires is examined, however, one finds a large number of affirmative answers on the Thurstone test tends to give a high 'neurotic' score; while a large number of like answers on the Northwestern test tends to pile up a high 'extravert' score. The writers would not say that these tests do not measure

TABLE I

INTERCORRELATIONS AMONG VITAL, PERSONAL, AND MORPHOLOGICAL FACTORS

	North western	Thurstone	Ratings	Self-ratings	M & G	W & H	H/Wt	Vital Capacity	Blood pCO_2	pCO_2 ±	$[BH\text{-}CO_3]_b$	pH_a (alkalinity)
Northwestern (Extroversion)	+.67											
Thurstone (Neurotic tendencies)	+.17	+.85										
Ratings (Rated adjustment)	+.02	−.29										
Self ratings (Extroversion)	+.43	−.16	+.33									
M & G (Pyknic habitus)	+.03	−.02	+.15	+.09								
W & H (Pyknic habitus)	+.02	−.06	+.03	.00	+.49							
H/Wt	+.03	+.02	−.03	+.04	−.82	−.32						
Vital Capacity	+.06	−.08	+.17	+.20	+.11	+.05	−.36	+.92				
Blood pCO_2	−.17	+.07	−.15	−.16	−.07	−.06	+.06	−.07	+.74			
pCO_2 ± deviations	−.23	+.17	−.10	−.15	−.14	+.01	−.02	−.03	.00		+.10	
$[BHCO_3]_b$	−.04	+.06	−.02	.00	−.10	−.20	+.07	−.02	+.62	+.02	+.52	
pH_a (alkalinity)	+.11	−.05	−.06	+.26	+.01	−.01	+.04	+.08	−.64	+.06	−.18	+.79

The signs of the original coefficients have been changed to conform to the legend of this table

traits characteristic of instability and introversion, respectively. We may suggest, however, that one or both of them may measure, to an extent heretofore unsuspected, the tendency for an individual to answer 'yes' to a question—to identify himself, so to speak, with the items presented. This hypothesis should be subjected to experimental inquiry, and, if verified, it should be more seriously considered in the construction of such interrogatories.

The correlations of self-ratings and ratings-by-experimenter with the 'personality' inventories, tend to bear out the above assumption. The magnitude of the correlation between self-ratings and experimenters'-ratings (+0.33) indicates that both of these are measuring, to some extent, a similar trait. That experimenters'-ratings on adjustment are similar to stability as measured by the Thurstone inventory is shown by the correlation of —0.23 between these measures. Self-ratings correlate with Thurstone to the extent of —0.16, indicating that individuals who judge themselves to be introverts tend to have a high Thurstone score.

Relationships with the acid-base equilibrium. As a matter of laboratory procedure, blood-samples which did not give analytical checks to within 2% were discarded, and the *S*s were called back for a second sample. The reliability coefficients for blood-measures in Table I are correlations between determinations made on blood-samples collected one week apart, and thus represent actual individual variability as well as experimental error. Correlations between determinations made on the same

blood-sample are close to unity. The N used for our reliability coefficients was 50. Of this number, 25 cases were from *Ss* in this experiment; the remainder were taken from unpublished data on other male students previously collected by N. W. Shock.

The correlations between measures of personality and certain factors in the acid-base balance are particularly consistent. Introversion (Northwestern), introversion (self-ratings), neurotic tendencies (Thurstone), and poor adjustment (experimenters'-ratings) are all associated with high CO_2-tension, with r's ranging from 0.07 to 0.17. (Cf. note below the table regarding interpretation of signs.) These CO_2 tensions are reflected in pH determinations.

Preliminary examination of extreme groups from the 'personality' tests showed that extreme introverts and neurotic individuals tend to have a much greater variability in carbon-dioxide tension than the better adjusted individuals. In order to obtain a measure of this tendency throughout the entire range of *Ss*, carbon-dioxide-tension measurements were transformed into standard scores and correlated without regard to sign ($pCO_2\pm$ or carbon-dioxide tension deviation scores). It was found that all four 'personality' measures correlated with $pCO_2\pm$ scores in the same direction as with pCO_2, the r's ranging from 0.10 to 0.23.[17]

Taken as a whole, these correlations indicate a consistent tendency for less adequately adjusted individuals, as measured by our tests and ratings, to have irregular CO_2-tension, this irregularity being most frequently in the direction of increase. The tendency means physiologically that this group tends to fall into Starr's category of *sub-breathers* (p. 467 above). The evidence further indicates that these persons are more apt to give aberrant pCO_2-values, and also that well-adjusted individuals are a more homogeneous group. In the specific situation of blood-letting, some less adequately adjusted persons tend to be sub-breathers, while others tend to be super-breathers. This produces a tendency toward a bimodal distribution of pCO_2.

Physique relationships. The morphological indices, while in some agreement among themselves, did not correlate consistently with any of the 'personality' measures, and their relation to vital capacity was surprisingly low. Nor were they related to the blood measures, except that both indices of body-build tend to show that asthenics have a higher bicarbonate content of the blood serum than pyknics. This is an interesting physiological-anatomical finding that needs verification and further study.

Discussion of Results

A significant result of this study is that pH and CO_2-tension showed consistent correlations with the 'personality' measures, while $BHCO_3$ did not seem related thereto. Carbon-dioxide tension and its reflection in pH

[17] Means and sigmas of the acid-base factors were; pH (hydrogen ion concentration) 7.389 ± .038; pCO_2 (carbon-dioxide tension in mm. of mercury) 44.75 ± 3.72; $BHCO_3$ (concentration of bicarbonate in milli mols per liter of serum) 26.21 ± 1.74).

are influenced more by respiratory regulation, which is a relatively labile system, than by metabolic changes, which are relatively stable. For instance, blood-pH may be increased from 7.40 to 7.60 by 10 min. of overbreathing, and will return to normal within 2 min. after cessation of overbreathing. Increasing pH by oral administration of sodium bicarbonate requires 1½-2 hr. to reach a maximum, and recovery takes place only after 8-10 hr. (cf. footnote 7). In the first case the displacement and adjustment are respiratory, while in the second they are metabolic.

Whether the existing relationships between acid-base factors and 'personality' indicate a direct or indirect causal relationship is a question which cannot be decided on the basis of our present knowledge. If the relationship is direct, we believe that the presence of correlation between respiratory factors in the acid-base mechanism and measures of personality may best be explained on the assumption that the causal relationship is from personal characteristic to physiological alteration, rather than in the reverse direction. In short, we believe that the correlations found tend to indicate the possibility that persons who are unadjusted, excitable, 'nervous' or introverted, alter the acid-base balance of their blood through respiratory changes when confronted with a new situation. The lack of correlation between the 'personal' measures and the metabolic factors in the equilibrium tends to throw some doubt on the assumption that acidity or alkalinity of the individual bears a causal relation to his behavior.

The above explanation could account for all the important data on personality-acid-base relationships in normal *Ss*, with but one exception, *i.e.* Rich's correlation of —0.30 between excitability and titrable acidity of urine (cf. page 467, sup.). It should be remembered, however, that urine is a waste-product of the body which is greatly influenced by such factors as diet and activity, and consequently may or may not give an index of the acidity or alkalinity of the body itself. In fact, under experimental conditions, Haldane, and Palmer-Van Slyke, have demonstrated that the urine may be either more or less alkaline than the circulating blood.[18]

Regardless of the direction of the causal dependence, it seems quite probable that irregularity of physiological function, including the acid-base mechanism, tends to be associated with instability in terms of behavior. Hammett[19] has submitted some evidence on this problem in a study of the nitrogen metabolism of twenty individuals. Although he had no adequate measures of behavior, he seemed to find evidence for a relationship between variability in the physiological measures and instability of personal characteristics. Similar results were found in a study of the acid-base

[18] J. B. S. Haldane, Experiments on the regulation of the blood's alkalinity, *J. Physiol.*, 55, 1921, 265-276; W. W. Palmer and D. D. Van Slyke, Studies of acidosis: IX. The relationship between alkali retention and alkali reserve in normal and pathological subjects, *J. Biol. Chem.*, 32, 1917, 499-508.

[19] F. S. Hammett, Observations on the relation between emotional and metabolic stability, *Amer. J. Physiol.*, 53, 1921, 307-311.

mechanism, the biochemical data of which were presented by Shock and Hastings (cf. footnote 7, p. 468 *supra.*). For attacking this problem repeated physiological and psychological measurements must be taken on the same group of *Ss*. Consequently results are slow in accumulating.

The writers are convinced that the greatest weakness of the present experiment, as well as of others of a similar nature, lies in the adequacy of the alleged measures of 'personality' used. Where validity is of primary importance, as in the present case, we feel that a system of ratings is the best method at present available. Only such personal traits should be considered as are capable of accurate verbal delimitation and reliable judgment by a number of raters.

SUMMARY

(1) Small but consistent correlations indicate that an instable personality, as determined by the procedures used in this study, is associated with respiratory irregularity, usually in the direction of sub-breathing.

(2) No large or consistent correlations were found between morphological indices and the measures of personality used in this study.

(3) No large or consistent correlations were found between morphological indices and the acid-base equilibrium of the blood.

(4) The nature of the relationships found tends to indicate that acid-base factors, in so far as they are related to personal characteristics, may be the result rather than the cause of these characteristics.

AN OBJECTIVE STUDY OF BINOCULAR VISION

By ROBERT H. PECKHAM, Temple University

The traditional explanation of binocular fusion rests on the assumption of corresponding retinal points. The mathematical definition of these points has been given by Helmholtz, who wrote, "Identisch sind solche Punkte beider Gesichtsfelder, welche gleiche Höhenwinkel und gleiche Breitenwinkel haben."[1] Hering utilized the principle of corresponding points in his theory of "space-feeling."[2] His theory of stereopsis, however, had its origin in the view advanced by Panum that binocular fusion did not result from point-to-point correspondence, but rather from point-to-area correspondence. Panum wrote, "ein jeder empfindende Netzhautpunkte des einem Auges einen correspondirenden Empfindungskreis . . . in anderen Auge hat."[3]

Since Hering most of the investigators of binocular vision have accepted the nativistic hypothesis, and have relied upon the assumed identity of the retinal structure for the explanation of binocular fusion. In addition to this a relationship between accommodation and convergence has been assumed. The methods employed to test this relationship have proceeded on the assumption that binocular fusion involves the stimulation of corresponding points. The present study is concerned with this latter assumption.

PROCEDURE

In order adequately to investigate the binocular response mechanisms, three factors must be considered: (1) the separate stimulation of each eye; (2) the subjective report of fusion or diplopia; and (3) the objective measure of eye-movements.

To stimulate each eye separately with identical patterns, a modified Helmholtz telestereoscope was used (Fig. 1). The distance from *S*'s eye to the pictures was 50 cm. Telescopes were added with which to measure eye-movements.

In using this stereoscope, as in using any other, it was found that unless the pictures were placed in a certain position, called the phoric position, they would

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[1] H. von Helmholtz, *Handbuch der Physiologischen Optik,* 3, 1910, 345.

[2] *Ibid.,* 444.

[3] P. L. Panum, Ueber die einheitliche Verschmeltzung verscheidenartiger Netzhauteindrüke beim Sehen mit zwei Augen, *Arch. f. Anat., Physiol., u. wiss. Med.,* 1861, 83.

frequently fail to fuse into a binocular single image. This position was determined by placing unlike pictures in the stereoscope and moving them about until *S* reported them to appear in the same spacial position, as shown in Fig. 2. This phoric position represents to a degree the foveal projection, and yields an index which can be used in subsequent estimations of the foveal change of position.

Following the determination of the phoric position one of the two identical pictures was moved horizontally in the stereoscope. It was found that during a limited amount of displacement the pictures would remain fused, but if the picture was

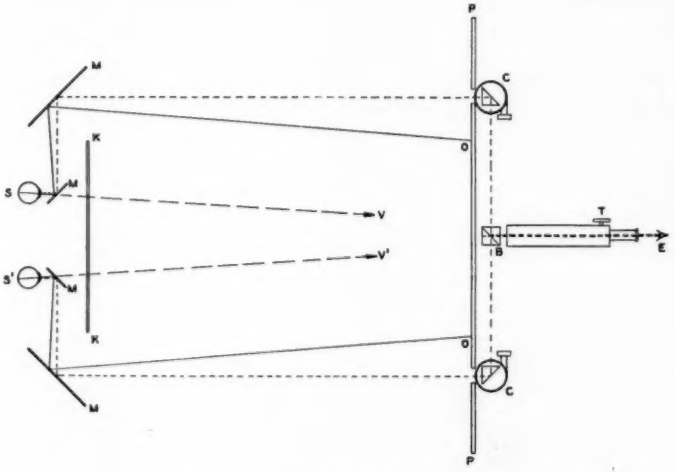

FIG. 1. OPTICAL PLAN OF THE STEREOSCOPE
M = mirrors; K-K = septum; P-P = object plane; *S*, *S'* = subject's eyes; O, O' = objects; V, V' = visual axes; C = calibration devices; B = beam-splitting prism; T = telescope; *E* = experimenter.

moved far enough, *S* reported diplopia. The horizontal displacement which can be tolerated before diplopia is fairly constant with a given *S* under rigidly constant conditions, but varies considerably from *S* to *S*. Under the conditions of this investigation, the range of displacement of one object, without diplopia, was from 3° to 10° inwards and outwards, the other object remaining stationary.

This procedure is optically identical with the ophthalmic procedure in estimating "ductions."[4] In the subsequent discussion, the term Abduction, or "out-duction," refers to the displacement of one of the stereoscope's objects outwards, into the field of the nasal retina, and the term Aduction, or "in-duction," to the inward displacement toward the field of the temporal retina. This corresponds, respectively, to the placing of base-in and base-out prisms before the eyes. It is generally assumed that the eye follows such movement of an object in the stereoscope, as long

[4] G. E. de Schweinitz, *Diseases of the Eye*, 1921, 76.

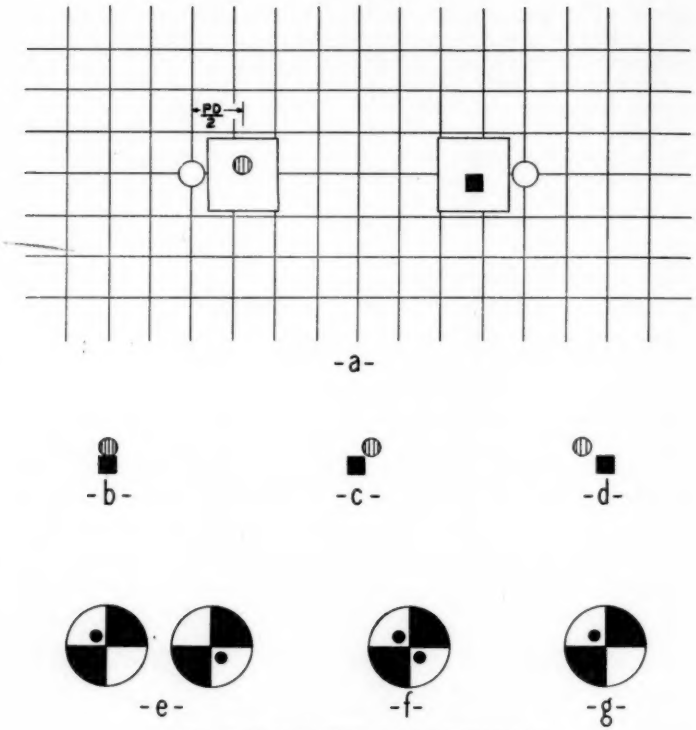

FIG. 2. OBJECTS USED IN THE STEREOSCOPE

a = phoria test objects in initial position; b = appearance of objects with orthophoria; c = appearance with exophoria; d = appearance with esophoria; e = fusion objects; f = binocular appearance of fusion objects; g = suspension (right eye).

as no diplopia occurs. For example, if one object is moved for 5° while the other remains stationary, the one eye will rotate for 5° and the other eye will remain fixed. It is assumed that diplopia occurs when the limit is reached beyond which the independent and dissociated eye-movement cannot extend, presumably because of the interference of convergence and accommodation. This in turn causes diplopia through the stimulation of non-corresponding points.[5]

[5] The theory of convergence-accommodation function is described in the following: F. C. Donders, *Anomalies of the Refraction of the Eye,* 1864, 110-126; Helmholtz, *op. cit.,* 3, 1910, 50; C. Worth, *Squint,* 1921, 3; G. E. de Schweinitz, *op. cit.,* 46; A. Ames, and G. Glidden, Ocular Measurements, *Trans. Sec. Ophthal., Amer. Med. Ass'n.,* 1928, 11-47; E. F. Tait, A reciprocal reflex system in the accommodation-convergence relationship, this JOURNAL, 45, 1933, 647-662.

The fallacy involved lies in the assumption that the report of continued fusion is evidence of accurate fixation and of the stimulation of corresponding points. To test the theory we need only measure the amplitude of the eye-movement. A total of 103 *Ss* have been examined, undergoing the phoria test, followed by at least two measures each when the object was displaced horizontally, both inwards and outwards. In each case the amount of image-displacement was one degree less than that which was found to produce diplopia, in order that the *Ss* would not suffer diplopia during

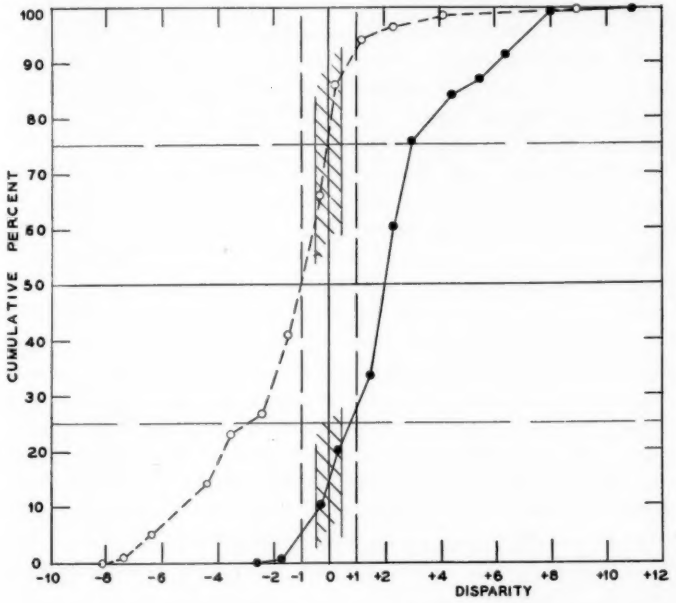

FIG. 3. CUMULATIVE DISTRIBUTION OF TOTAL BINOCULAR DISPARITY
Solid line = induction; broken line = outduction; temporal disparity plus, nasal disparity minus.

the measurements. For the purpose of calibration, we recorded the movement of one eye alone in following the same displacement of the object while the other eye was covered.

If the fixation during the binocular fusion of the displaced images had been accurately foveal, the measure of the eye-movement during the fusion would have been the same as the monocular measure. If there were a difference, it would represent the discrepancy in binocular fixation for that eye. If the eye before the stationary object moved at all, its movement would also represent discrepancy. When such discrepancies occur, their algebraic sum represents binocular disparity, and is evidence of fusion when non-corresponding points were stimulated.

Results

The distribution of the amounts of binocular disparity, by cumulative percentages, is shown in Fig. 3. When the object was moved inwards (the solid line) it is seen that 14% of the *Ss* maintained binocular fixation within a disparity of plus or minus one half degree. This is within the experimental error of the apparatus. Of the *Ss*, 22% showed disparity within an area 2° broad, which is approximately the limit proposed by the Panum-Hering theory, 90% were included in a range of disparity from +7.5° to —1°. With outduction, that is, when one object was moved outwards, 23% of the *Ss* showed an accuracy of fixation within the experimental error, 44% showed disparity within an area 2° broad, and 90% were included within a range of disparity from +1.5° to —6.5°.

These data indicate that binocular fusion was reported when the images stimulated points failing to be anatomically corresponding by as much as 10°, in the extreme. Empirical examination of binocular fixation, in this situation, reveals that the theory of binocular fusion exclusively by the stimulation of corresponding retinal points is fallacious.

The usual nature of the response of the eyes to the displacement of the object in the stereoscope was this: the eye before the moving object moved in the same direction as the object, but through a smaller angle. The eye before the stationary object usually moved in the same direction as the other eye. This resulted in the stimulation of non-corresponding points by the images of the objects. When the assumption that the phoric position of the eye is an index of the foveal position is maintained, these responses frequently resulted in the stimulation of retinal points on opposite sides of the foveas. We must conclude that binocular fusion is possible when the stimulation is upon greatly disparate points, upon either side of the foveas.[6]

Pierson's coefficient of correlation for the odd versus the even measurements of disparity is +0.68, ±.04. This indicates only that the reaction of a given *S* was characteristically in the same direction although varying in amount. Sigma for induction is 2.3°, and for outduction is 2.8°. This variation of the response lends credence to the hypothesis that binocular fixation is the result of two approximate monocular fixations.

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[6] Even while denying this assumption, we may still conclude that binocular fusion is possible when disparate retinal points are stimulated, since at some time during the experiment such points are stimulated. Foveal fixation may have been accomplished in the beginning, at the end, in the middle, or not at all. We have no direct evidence of the foveal position.

Conclusions

The results of this study may be summarized as follows:

(1) Fusion is possible when disparate or non-corresponding retinal points are stimulated.

(2) No conclusions can be drawn from investigations of the relation between convergence and accommodation unless measurements are included of the eye-movements.

(3) The results of the studies which have been made under the assumption that binocular fusion is evidence of the stimulation of corresponding retinal points are open to question.

(4) The theory of a convergence function, or theoretical convergence-accommodation relationship, that depends upon the assumption of binocular fusion exclusively through the stimulation of corresponding retinal points, or near corresponding points, is fallacious.

(5) The theory that the state of convergence in binocular fixation may be accomplished by two separate and independent monocular fixations is favored.

THE PERCEPTION OF SMALL DIFFERENCES AS AFFECTED BY SELF INTEREST

By J. M. STEPHENS, Johns Hopkins University

The problem of this study is most conveniently presented by way of a review of a previous experiment. In that experiment,[1] *S* sat facing a beaverboard screen. His hand, holding a stylus, projected through a hole in the screen toward a target which he could not see. He was told that upon the occurrence of a signal he was to make one hit at the target, trying to find a certain spot on it. He was told also that hitting the right place would cause a bright light to flash, while hitting any other place would cause a dim light to flash. (The bright and the dim lights arose from the same lamp, the difference in brightness being controlled by the amount of current passing through the filament.) Immediately after each hit *S* announced "right" or "wrong," according to whether a bright or a dim light had flashed. He then received the signal for the next hit.

In such a situation *S* has a chance to err in his interpretation. If the difference in the brightness is not very marked, he may say "right" when the dimmer light flashes, or "wrong" when the brighter light flashes. By pure chance each of those errors would be equally probable, since there were approximately as many occurrences of the bright light as of the dim. In actual fact, however, there were only 9 instances in which the *S*s interpreted a bright light as meaning "wrong," and 417 instances in which they interpreted a dim light as meaning "right."

A disparity of this size suggests that in this type of activity the *S*s are prone to interpret an ambiguous signal to their own advantage. When there is any doubt they interpret the signal to mean that they are right. One must add that in this case the matter of deception can hardly enter. Since *S* knew that *E* had seen where he hit the target, he would surely hesitate to say 'right' if he actually thought the signal meant 'wrong.' The phenomenon seems to be one of interpretation or judgment.

The present study is merely a further investigation of the same tendency. The experiment was conducted with 34 *S*s in a regular class examination in educational psychology. The examination was of the true-false type, using a separate answer-sheet. *S* read the question or statement on the question-sheet. If statement No. 1 were true, he put a circle around the T opposite Item 1 on the answer sheet (see Fig. 1). If it were false he put the circle around the F. Sheet 2 was stapled (one staple at each corner) to Sheet 1 (the answer sheet). Carbon paper was placed between them so that encircling T of any answer on Sheet 1 would leave a carbon impression of the circle on the left-hand character of the corresponding item on Sheet 2. The presence of the carbon paper and of Sheet 2 was perfectly obvious and no attempt was made to disguise it. An explanation was promised at the conclusion of the test.

When all *S*s had finished the test, *E* announced that he was trying to devise an answer-sheet by which a student could discover his mistakes as soon as possible after

* Accepted for publication November 9, 1934. From the Department of Education.

[1] J. M. Stephens, The influence of different stimuli upon preceding bonds: An examination of the law of effect, *Teach. Coll. Contrib. Educ.*, 1931, (no. 493).

he finished his test. In the use of this new answer-sheet the student would hand in the original (Sheet 1) as soon as he finished the test, retaining Sheet 2. He would thereupon receive a key (Sheet 3) by which he could correct his paper from Sheet 2. Now, if it were very easy to mark Sheet 2 from the key, a neighboring *S* who had not finished might be tempted to glance at it. Consequently it was important to work out a sheet which *S* could interpret with assurance but which would be difficult to interpret from a surreptitious glance at a distance. In this test *E* was trying to see just how difficult that interpretation could be without being impossible. In the case of some of the items it would be very easy for *S* to tell whether he was right or wrong. In other cases it would be very hard. *E* would check all papers afterward.

The *S*s then handed in Sheet 1 and the question papers, and received the key (Sheet 3). (The question papers were handed in so that the *S* could not look up a question and thus estimate whether the answer was likely to be right.) They were

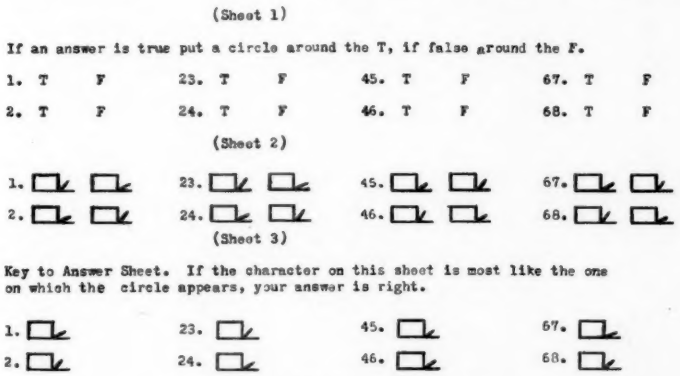
(Sheet 1)
If an answer is true put a circle around the T, if false around the F.
1. T F 23. T F 45. T F 67. T F
2. T F 24. T F 46. T F 68. T F
(Sheet 2)
1. 23. 45. 67.
2. 24. 46. 68.
(Sheet 3)
Key to Answer Sheet. If the character on this sheet is most like the one on which the circle appears, your answer is right.
1. 23. 45. 67.
2. 24. 46. 68.

FIG. 1. SAMPLES OF THE QUESTION-SHEETS

then told to mark their answers 'right' or 'wrong' according to the instructions at the head of Sheet 3.

By this means it was hoped that *S* would regard the matter as a genuine test of his ability to perceive small differences, and that, being perfectly sure that the papers would be checked, he would not be tempted to deceive. We wished him to decide which of the two characters of Sheet 2 was more like the character of Sheet 3, knowing that one decision would increase his score, the other decrease it. In the case of any item he would not know *what* statement he had marked 'True' or 'False,' unless he could remember the content of the questions by number.

In order to secure corresponding judgments from disinterested *S*s to use as a control, 19 other students were given Sheets 2 and 3 only and asked to judge whether the characters on Sheet 3 (the key) were more like the first or the second of the characters on the corresponding items of Sheet 2. Since neither decision had any advantage for the *S*s, the results from this group should give us a fair indication of disinterested judgments.

It will be noticed that the chief difference between the two characters of any given item on Sheet 2 lies in the position of the short fifth line in the open right angle. In the first character of Item 1, the angle between this line and the horizontal base is 51°. In the second character the corresponding angle is 27°. (Henceforth the characters will often be identified by this distinctive angle. For instance 27° will refer to a character in which this distinctive arm makes an angle of 27° with the base.) In Item 2 the order of the two characters is reversed. Each item on Sheet 2 consists of a 27° character and a 51° character. In every second item the 27° character comes first, in the others the 51° character comes first. The corresponding angle of the characters on the key (Sheet 3) is more variable. In the case of 9 items this angle is 27°, in the case of 17 items it is 31.5°, in the case of 30 items it is 37°, in the case of 14 items it is 43.5° and in the case of 7 items 51°. In one item on the key the angle was inadvertently omitted. Examples of a 27° degree

TABLE I

NUMBER AND PERCENTAGE OF THE VARIOUS GROUPS FAVORING THE RESPECTIVE ANGLES

Group	Judgment	Size and log. values of distinctive angle in key				
		27° .43	31.5° .50	37° .57	43.5° .64	51° .71
A	No. like 27°	234	372	268	19	2
27°	No. like 51°	7	40	153	93	34
	%like 27°	97±0.7	90±1.0	64±1.6	17±2.4	6±2.7
B	No. like 27°	168	281	279	38	4
neutral	No. like 51°	3	41	291	227	129
	%like 27°	98±0.7	87±1.3	49±1.4	14±1.4	3±1.0
C	No. like 27°	53	109	129	14	7
51°	No. like 51°	9	47	429	343	192
	%like 27°	85±3.0	70±2.5	23±1.1	4±0.7	4±0.9
A–B	% like 27° only	−1±1.0	3±1.6	15±2.2	3±2.8	
B–C	" " " "	13±3.1	17±2.8	26±1.8	10±1.6	
A–C	" " " "	12±3.1	20±2.7	41±1.9	13±2.5	

character on the key appear in Items 1, 24, and 67. Examples of a 51° character are given in Items 2 and 23, while Items 45, 46, and 68 illustrate a 37° character.

From the purely perceptual point of view the task of all *S*s with respect to any one item is the same. Each *S* is to decide whether the character of a given item on the key is more like the first or the second character of that item on Sheet 2. But from the self-interest point of view there will be 3 different groups with respect to that item. The members of Group A will have a circle on the 27° character of Sheet 2. If the key is nearest like the 27° character the item will be right; if not, wrong. The members of Group B (the neutral or control group) will have no circle on either character and no score to be affected. The members of Group C will have a circle on the 51° character. If the key is nearest like this 51° character the item will count as right; otherwise as wrong. Will the members of Group A be more likely than the neutral group to decide that the key resembles the 27° character? Will Group C consider it to be like the 51° character?

The preceding statement indicates the manner of treating the data. All the papers

were, in effect, sorted for each item. In Group A were all the papers in which the 27° character of that item had been circled. Group B consisted of the neutral group. In Group C were all the papers in which the 51° character had been circled. Obviously the composition of Groups A and C could be different for different items. An *S* whose mark was on the 27° character for Item 1, thus placing him in Group A, might have marked the 51° character of Item 2 and thus be in Group C for that item. The composition of Group B is constant throughout.

All the items having similar key characters were combined and the results totaled. The totals for each type of item are given in Table I.

The first row of Table I indicates the angle which was common to all the key items treated in that column. Thus under column headed "27°" are given the results of all the items whose key was a 27° character. In column headed "43.5°" are given the results for all the items whose key was 43.5° character. The log values have no bearing on this study. They merely indicate that the angles were in an approximate geometric series. The distinction between Groups A, B, and C should be sufficiently clear from both the table and the preceding discussion. The third row of figures indicates that of all the cases where the circle was on the 27° character, 234 judgments considered the 27° character on the key to be like the 27° character on Sheet 2; 372 judged a 31.5° key character to be like a 27° character on Sheet 2; 268 judged a 37° key character to be like a 27° character on Sheet 2, and so forth. The last three rows give various differences between the groups except in the case of the last column in which case the differences are obviously insignificant.

The results show that the neutral group made a negligible number of errors of judgment at the extremes. In only 2 or 3% of the cases was an angle of 27° judged to be more like an angle of 51° or vice versa. The angles of 31.5° and 43.5° each show an error of 13 or 14%. The 37° character appears to be perfectly ambiguous for the neutral group. This, of course, would be the expectation if judgments of this type followed the Weber-Fechner law, since 37 is the geometric mean of 27 and 51. At any rate, this 37° character was just as likely to be judged "more like 27°" as "more like 51°."

It will be observed that this 37° character, which was perfectly ambiguous for a neutral or disinterested group, was judged differently by the "interested" groups. Group A, whose mark was on the 27° character, tended to judge the ambiguous 37° character to be like the 27° character; Group C tended to judge it to be like the 51° character. The same tendency is indicated at other places in the case of Group C. In the case of each angle except 51° the judgments of this group differed significantly from those of the neutral group, and always in the direction of self-interest.

The more consistent bias of Group C is interesting. No very sure explanation presents itself. The bias cannot be due to differences in the membership of Groups A and C since, as mentioned above, every member would appear sometimes in Group A and sometimes in Group C. Such bias may be due to some such artifact as a different point of reference. The 51° angle may be judged with reference to the vertical (since it has passed the 45° point), and this may permit a greater toleration of error. While the results of the neutral groups showed no such greater toleration, it may be that when any factor of bias is present it may be more free to work in those comparisons which refer to the vertical.

It will be noticed that these results are derived from all the individuals lumped together. From such results we can say that the tendency to make judgments in accord with self-interest is present in the group as a whole, but not that it is present in every member, nor how much it varies from one person to another. It may be that only a few individuals possessed this tendency and that these few pulled the group average in the direction of self-interest.

In order to secure individual scores of significant reliability one would have to make sure in the first place that all the members of a given number of items were equally ambiguous. That is, we should be sure that each of the 37° characters, for instance, was perfectly ambiguous. So far, of course, we have only made sure that *on the average* the 37° characters were almost perfectly ambiguous. But there may be slight differences from one 37° character to another. These differences may be actual differences in the size of the angle, or psychological differences, arising from the size of the angle preceding the angle in question. For instance, in the case of the latter point, if the 4 characters preceding the 37° character of Item 45 had been 51° characters, and the 4 characters preceding the 37° character of Item 68 had been 27° characters, then the 37° character of Item 45 might *seem* smaller than the 37° character of Item 68.

In order to make the individual scores reliable we should, first, have to give the neutral test to a group large enough to determine whether all the 37° characters were equally ambiguous. This would likely involve about 200 neutral *S*s. We should then need a number of such equally ambiguous characters sufficient to enable us to get rid of the chance element in the choices of each individual. That number cannot be determined in general. If, however, an individual judged 65% of the ambiguous items to correspond with the character he had marked (the neutral group, of course, judging only 50% of such characters to correspond), his self-interest score would apparently be 15%. In order to be reasonably sure that such a difference were not due to pure chance, it should be based on almost 200 choices.[2] If, of course, the value were greater than 65%, the number of choices necessary to establish significance would be less than 200. But with fewer than 175 or 200 choices we could not be sure that any individual score between 35% and 65% was due to real bias or to chance errors. In our study we have neither enough *S*s in the neutral group to be sure that all our 37° characters are equally ambiguous, nor have we for any individual enough even presumably equally ambiguous choices to eliminate chance errors. Consequently individual scores are out of the question.

Conclusion

From the results of the group as a whole we may conclude that when an *S* is asked to decide whether his response is right or wrong and when the key by which he judges his responses is ambiguous, then he tends to call his response correct. He does this even when he is fully aware that his decisions will be checked by the experimenter.

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[2] With 200 choices, 65% has standard error of 3.3%. But by pure chance (*i.e.* no bias) we should get 50% selection which on the basis of 200 choices has a standard error of 3.5%. Our value of 65% differs from chance then by 15% with a standard error of 4.8%, which is just slightly above the minimum criterion of significance.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXXI. Adaptation of Warm Spots Under Continuous and Intermittent Stimulation

By Solomon Aronoff and Karl M. Dallenbach

Many experiments have been reported on adaptation to an areal temperature stimulus,[1] but none, as far as we have been able to discover, on adaptation to a punctiform stimulus acting on a single temperature spot.[2] The areal experiments indicate that temperatures close to physiological zero adapt completely. With other temperatures, adaptation is partial in the sense of blunting or dulling, an intensive change without qualitative disappearance; "it seems to begin rapidly and then continue at an ever decreasing rate, like an asymptote approaching its limit."[3]

Whether a single temperature spot adapts completely or only partially is a question of importance in thermal theory. The purpose of the present study was to determine the answer for warm spots. We proposed to begin with temperatures markedly above physiological zero, which have proved to be most unfavorable to areal adaptation, and to bracket to lower and more favorable temperatures as the turn of the experiment demanded.

Method

Observers. Our *O*s were 3 undergraduates, Miss Selma Karp (*K*), Mr. V. H. Dropkin (*D*), and Mr. H. A. Levine (*L*). Being untrained in the observation of

* Accepted for publication May 25, 1934.

[1] K. G. Holm, Die Dauer der Temperaturempfindungen, *Skan. Arch. f. Physiol.*, 14, 1903, 242-249.

Andreas Voigt, Über die Beurteilung von Temperaturen unter dem Einfluss der Adaptation, *Zsch. f. Psychol.*, 56, 1910, 344-355.

Edwina Abbott, The effect of adaptation on the temperature difference limen, *Psychol. Monog.*, 16, 1914, (no. 68), 1-36.

E. A. K. Culler, Thermal discrimination and Weber's law, *Arch. Psychol.*, 13, 1926, (no. 81), 7 f., 40 ff., 65, 82, 102 f., 105-108, 113.

Elof Gertz, Psychophysische Untersuchungen über die Adaptation im Gebiet der Temperatursinne und über ihren Einfluss auf die Reiz- und Unterschiedsschwellen, *Zsch. f. Sinnesphysiol.*, 52, 1921, 1-51, 105-156.

Erich Hummer, Die Veränderung des Systemes der Temperaturempfindungen durch die Adaptation, *Arch. f. d. ges. Psychol.*, 57, 1926, 305-394.

Helmut Hahn, Die psycho-physischen Konstanten und Variablen des Temperatursinnes: I. Die Konstanz der Empfindlichkeit der Temperaturnerven, *Zsch. f. Sinnesphysiol.*, 60, 1930, 162-197; II. Die Umstimmung der Erregbarkeit der Temperaturnerven, *ibid.*, 60, 1930, 198-232.

[2] Goldscheider very early made some casual observations upon the adaptation of the temperature spots, finding that cold and warm spots adapt completely and very quickly under continued and repeated stimulation. He writes: "Reizt man einen Kältepunkt wiederholt in kurzen Pausen mittelst des Cylinders, so nimmt er sehr bald diesen Reiz nicht mehr wahr. Ebenso wenn man den Cylinder eine gewisse Zeit lang dauernd mit dem Punkt in Contact lässt. Dasselbe lässt sich on den Wärmepunkten beobachten." (Alfred Goldscheider, Neue Thatsachen über die Hautsinnesnervern, *Arch. f. Anat. u. Physiol.*, Physiol. Sect., Suppl. Vol., 1885, 35).

[3] Culler, *op. cit.*, 41.

warmth, they were given a preliminary practice series of experiments to familiarize them with the quality of warmth and with its intensive gradations.

Apparatus and procedure. The dorsal surface of *O*'s left forearm, which was selected as the experimental tissue, was shaved at the beginning of every experimental hour so as to obviate the distraction which might be occasioned by the chance stimulation of the hairs. *O* then placed his arm in a plaster of Paris arm-rest which was cast especially for him. After stamping a 3 x 5 cm. grid map, with 2 mm. cross-section lines, upon the shaved area, *E* worked over it punctiformly[4] with a Cornell thermoesthesiometer.[5] Pressure of stimulation was kept constant by means of the spiral spring and gauge on the esthesiometer. Involuntary movements and tremors of *O*'s arm during stimulation were either eliminated entirely or reduced to a minimum by means of the arm-rests.

The following instructions were given to the *O*s.

"You will be stimulated with a warm point. Report when you experience warmth and then describe the intensity of your experience in terms of the following five-point scale: (1) weak, (2) mild, (3) moderate, (4) intense, (5) very intense."

The esthesiometer was heated by forcing through it under the pressure of the University water mains a stream of warm water that had passed through a copper coil immersed in a constant temperature bath. The rate of flow was adjusted with a needle-valve so that the temperature of the stimulus-point was constant within the limits of 43°-43.5°C.—the highest temperature we could use that was below the stimulus-range of paradoxical cold and heat.[6]

The warm spots located were marked on the grid map with India ink. After a 15-min. rest,[7] the main experiment began and *E* stimulated the marked spots either continuously or intermittently, and *O* observed and reported upon the process of adaptation.

In the continuous method, the esthesiometer was applied to a warm spot and kept there until *O* reported complete adaptation. Adaptation-time was measured in 1/5 sec. with a stop watch. In the intermittent method, which was used after the continuous method, the esthesiometer was applied successively to a warm spot until complete adaptation was indicated. *E* counted silently "one, two, three, four" in rhythm with a silent seconds-pendulum suspended back of *O*, and applied the stimulator to a warm spot on the count of "one," removed it at the count of "two," held it off the skin during the counts of "three" and "four" while *O* gave an estimate of intensity, and then applied it again at "one," and so on until *O* reported thermal indifference at three successive stimulations—our criterion of complete adaptation in this method.

Spots stimulated. The spots used in the main experiment—20 for every *O* with each method of stimulation—were selected, from among those located and marked earlier in the hour, upon the basis of their initial level of intensity and in proportion

[4] For a description of this method see K. M. Dallenbach, The temperature spots and endorgans, this JOURNAL, 39, 1927, 413 ff.

[5] This stimulator is described by K. M. Dallenbach, Some new apparatus, this JOURNAL, 34, 1923, 92 ff.

[6] See E. Lowenstein and K. M. Dallenbach, The critical temperatures for heat and for burning heat, this JOURNAL, 42, 1930, 423-429.

[7] A period which Earhart and Dallenbach (The response of warm spots under successive stimulation, this JOURNAL, 45, 1933, 729) found to be adequate for complete recovery.

to the frequency of the various levels reported. *L* alone reported spots at all levels, while *D* reported no 'weak' spots (level 1) and *K* reported none that were 'weak' or 'very intense' (level 5). Tables I and II show, for the continuous and intermittent methods respectively, the number of spots of different initial intensities used for every *O*.

TABLE I
AVERAGE TIME (IN SECONDS) REQUIRED FOR COMPLETE ADAPTATION OF PUNCTIFORM WARMTH

O		Initial intensity-level of the spots					All of the spots
		1	2	3	4	5	
D	No. spots	0	2	8	6	4	20
	Av.	—	7.3	11.9	11.0	18.0	12.4
	m.v.	—	1.1	4.6	3.8	3.8	4.8
	Range	—	6.2–8.4	2.6–25.6	6.4–21.4	12.2–23.6	2.6–25.6
	m.v./av.	—	15.1%	38.7%	34.5%	21.1%	38.7%
K	No. spots	0	4	10	6	0	20
	Av.	—	7.6	9.0	23.2	—	13.0
	m.v.	—	2.7	4.9	20.6	—	9.5
	Range	—	3.4–12.0	3.0–20.4	5.8–68.4	—	3.0–68.4
	m.v./av.	—	35.5%	54.4%	88.8%	—	73.1%
L	No. spots	0	8	10	0	0	20
	Av.	—	20.5	27.9	—	—	24.9
	m.v.	—	14.8	10.9	—	—	13.1
	Range	—	5.8–67.2	9.8–68.2	—	—	5.8–68.2
	m.v./av.	—	72.2%	39.1%	—	—	52.6%

Instructions. For the continuous method, the following instructions were given the *Os*.

"At a 'Ready, Now' signal a warm spot will be stimulated. Describe the temporal course of your experience, giving particular emphasis to the report of the intensive changes. Relax and remain as motionless as possible during the course of the experiment. Close your eyes at the signal and keep them closed until told to open them."

For the intermittent method, the instructions were as follows.

"At a 'Ready, Now' signal a warm spot will be stimulated repeatedly for short intervals of time. During the intervals between stimulations, report the intensity of the preceding warmth in terms of the 5-point intensive scale. Relax and remain as motionless as possible. Close your eyes at the signal and keep them closed until told to open them."

RESULTS

Adaptation. The results with the continuous method are given in Table I and those with the intermittent method in Table II. As the tables show, complete adaptation was reported by all the *Os* in all the experiments.[8]

[8] Since we obtained complete adaptation at the highest stimulus-temperatures available adequate only to warmth, it was thought to be unnecessary for the purposes of the present study to bracket to lower temperatures. Complete adaptation of he separate warm spots was adequately demonstrated. If the higher temperatures—those most unfavorable to areal adaptation—yielded to complete adaptation, lower temperature may safely be assumed to do likewise. Whether adaptation-time is conditioned by stimulus-temperature is a problem now set for solution.

The adaptation-times, however, vary considerably. For different spots they vary from 2.6—25.6 sec. for *D*, 3.0—68.4 sec. for *K*, and 5.8—68.2 sec. for *L*. The average is twice as large for *L* (24.9±13.1 sec.) as for *D* (12.4±4.8 sec.) or *K* (13.0±9.5 sec.). Adaptation-time is shortest at the lowest intensity-level (7.3, 7.6, and 20.5 sec. for *D*, *K*, and *L* respectively), of intermediate length at the intermediate levels, and longest at the highest levels (18.0, 23.2, and 27.9 sec. respectively).

TABLE II

AVERAGE NUMBER OF 1-SEC. STIMULATIONS REQUIRED FOR COMPLETE ADAPTATION OF PUNCTIFORM WARMTH

O		Initial intensity-level of the spots					All of the spots
		1	2	3	4	5	
D	No. spots	0	2	13	5	0	20
	Av.	—	18.5	32.9	31.2	—	31.0
	m.v.	—	6.5	22.7	10.2	—	17.0
	Range	—	12–25	7–123	10–56	—	7–123
	m.v./av.	—	35.1%	69.9%	32.7%	—	54.8%
K	No. spots	0	1	7	12	0	20
	Av.	—	10	21.6	32.8	—	27.8
	m.v.	—	—	16.0	18.1	—	13.3
	Range	—	—	4–58	9–103	—	4–103
	m.v./av.	—	—	74.1%	55.2%	—	47.8%
L	No. spots	1	3	3	5	8	20
	Av.	33	21.3	23.6	26.0	49.0	34.5
	m.v.	—	7.1	5.6	6.0	22.8	14.6
	Range	—	15–32	17–32	15–41	17–100	15–100
	m.v./av.	—	33.3%	23.7%	23.5%	46.5%	42.3%

Similar results were obtained with the intermittent method (Table II). The number of 1-sec. stimulations (made at 3-sec. intervals) required for three successive reports of thermal indifference—the criterion for complete adaptation in this part of the study—varied with the spot used: from 7–123 stimulations for *D*, 4–103 for *K*, and 15–100 for L. The average number was 31.0±17.0 for *D*, 27.8±13.3 for *K*, and 34.5±14.6 for *L*. It was smaller at the lower than at the higher intensity-levels.

If the number of 1-sec. stimulations in the intermittent method be counted as the time of adaptation under that method, the intermittent adaptation-times average larger than the continuous. The average time is longer by 18.6 sec. for *D*, 14.8 sec. for *K*, and 9.6 sec. for *L*. This result was not unexpected. The 3-sec. interval between successive stimulations extends the adaptation-time. The difference, however, is so small as to suggest that a prolonged after-effect, or a slow recovery of the endorgan, or both factors combined, are operating to obscure the effect of the intervals of rest.

Since adaptation-experiments have generally been limited to areal stimuli, we have little with which to compare our results. Our adaptation-times, m.v., and ranges of dispersion are, however, much smaller than those recently reported for

punctiform pain.[9] We may, therefore, state with definiteness that punctiform warm, in comparison to pain at least, adapts rather quickly.

Temporal course. The course of adaptation with a continuous stimulus, as our *Os* describe it, is not, like the curve of fatigue or the course of adaptation to pain, a gradual and continuous decline, but it is rather discontinuous and fluctuating. With continuous stimulation, however, so many changes are telescoped within an interval of a few seconds, that it is difficult for the *Os* to describe fully or adequately the process of adaptation. The *Os* themselves confess to the omission of many phases either because these were too brief for adequate noting or because of interference between the observation and the act of reporting. Indeed, all the *Os* were at times hesitant and uncertain in their reporting.[10] Experience does not halt in its course for report.

FIG. 1. DIAGRAMMATICAL REPRESENTATION OF THE TYPICAL COURSE OF WARMTH-ADAPTATION

The intermittent method of stimulation provides a more adequate opportunity for precise description. Every stimulation, it will be recalled, was followed by a description of the experience. If experience will not halt in its course for report, stimulation would. Moreover, the process of adaptation was extended in time. The results obtained with this method corroborate and clarify those obtained with the continuous method. That the course of adaptation is discontinuous is clearly shown in the following reports. The intensity-level at successive stimulations is indicated by the steps in our 5-point scale (0 = thermal indifference, and 5 = very intense).

D, (Exp. 15): 4, 4, 4, 3, 3, 1, 1, 0, 2, 2, 1, 0, 1, 1, 1, 1, 0, 0, 0.
K, (Exp. 20): 4, 4, 4, 3, 3, 2, 0, 3, 1, 1, 0, 0, 0.
L, (Exp. 9): 5, 5, 4, 5, 5, 4, 4, 3, 3, 4, 4, 3, 3, 3, 3, 3, 3, 3, 3, 2, 2, 0, 2, 2, 2, 1, 0, 2, 1, 0, 0, 1, 0,0,0.

The course of punctiform adaptation of warmth, graphically represented and depicted in Fig. 1, is similar to that described by Gertz for areal adaptation.[11] The intensity pulsates; it falls and rises several times before thermal indifference becomes relatively permanent. The successive phases decline at first gradually in intensity and then more and more rapidly. The recurring rises are, however, stepwise and abrupt. The succeeding phases, of which there may be from 2 to 4, are progressively weaker and shorter in duration (see Fig. 1).

Conditions affecting the time of adaptation. Adaptation-time is in part conditioned by the sensitivity of the warmth spots. This conclusion is borne out by the

[9] Burns and Dallenbach, *op. cit.*, 113.
[10] This stands in substantial agreement with the results obtained by Holm, *op. cit.*, 243.
[11] Gertz, *op. cit.*, 111.

fact that the averages of all the *O*s in both methods of stimulation vary directly with the intensity-level of the spots stimulated. Everything else being equal, low-level spots adapt sooner than high-level spots. But everything else is not equal, as the overlap in the variations, m.v., and ranges of dispersion of each intensity-level clearly indicates. Other factors may be found among the psychophysical conditions affecting the limen, for between our experiments and liminal determinations there is a similarity in method. Fatigue and ennui are not contributory factors; variations in adaptation-time were as great early in the experimental hour and during the early periods of the experiment as later.

Summary

The results of this study may be briefly summarized.

(1) In opposition to the results that have been obtained by areal stimuli, warm spots adapt completely under punctiform stimulation—whether continuous or intermittent—with maximal temperatures.

(2) Adaptation is rapid, the time varying with the sensitivity of the spot, with the *O*, and with the method of stimulation.

(3) The course of adaptation is discontinuous. It is marked by a pulsating intensive decline in which the successive phases are weaker in intensity and shorter in duration.

(4) Stimulation of the warm spots appears to have an after-effect of such duration as to obscure in part the effect of the 3-sec. intervals of rest in the intermittent method.

LXXXII. Adaptation of Cold Spots Under Continuous and Intermittent Stimulation

By Harold A. Levine and Karl M. Dallenbach

Our object in the present study was to duplicate with cold spots Aronoff and Dallenbach's experiment on the adaptation of warm spots.[1] We wished specifically to determine whether a single cold spot adapts completely under a stimulus-temperature that is markedly below physiological zero, because earlier studies have shown that low temperatures do not yield complete areal adaptation.[2]

Method

The present study was carried out at the same time as Aronoff and Dallenbach's study, with the same *O*s (*D*, *K*, and *L*[3]), and with similar apparatus, procedure, and instructions.

After a preliminary practice series which familiarized the *O*s with the quality of cold and with its intensive gradations, the procedure was as follows. The shaved

* Accepted for publication June 1, 1934.

[1] Solomon Aronoff and K. M. Dallenbach, Adaptation of warm spots under continuous and intermittent stimulation, this Journal, *supra*, 485-490.

[2] For a bibliography of these studies see *supra* 485, footnote 1.

[3] When *L*, the junior author and *E* in this study, acted as *O*, Mr. Solomon Aronoff conducted the experiments.

dorsal surfaces of *O*'s left forearm was placed in a plaster-cast arm-rest. A 2-mm. grid map (3 x 5cm.) was stamped on the skin and the cold spots within that area were located by the punctiform method and marked. The thermoesthesiometer used as stimulator was cooled by means of a stream of water forced through it. The water, drawn directly from the University mains was, because of a by-pass running full force, comparatively cold. The temperature was fairly constant during any one experimental period, but in different periods it varied from 8.5° to 17°C.[4] The stimulator was applied at a constant pressure and for a constant duration. The cold spots were located and marked, and a record was made of their initial intensity-levels as estimated by *O* on a 5-point scale: 1 = weak, 2 = mild, 3 = moderate, 4 = intense, 5 = very intense.

After a 20-min. interval of rest,[5] each of a selected number of spots was stimulated either continuously or intermittently until complete adaptation was reported. The spots—20 for every *O* with each method of stimulation—were selected, from among those marked at the beginning of the experimental hour, upon the basis of their initial intensity-level and in proportion to the frequency of their occurrence. *D* and *K* reported no initially "weak" spots (level 1); *K* and *L* reported none that was "very intense" (level 5). Tables I and II show the distribution of the intensity-levels that were used with every *O* both in the continuous and in the intermittent method.

In the continuous method, the stimulator (whose temperature varied between 14°–14.5°C. for *D*, 16.5°–17° for *K*, and 11°–16° for *L*) was applied to a cold spot and held there until *O*, who was instructed 'to describe the temporal course of his experience giving particular attention to intensive changes,'[6] reported complete adaptation. Adaptation-time was measured in 1/5 sec. by means of a stop watch. In the intermittent method, the stimulator (whose temperature varied between 11°–12°C. for *D*, 8°–11.5° for *K*, and 8.5°–11° for *L*) was applied successively to a cold spot until complete adaptation was indicated. It was held to the spot for 1 sec., off for 3, on again for 1, off for 3, and so on, until *O*, who was instructed to report, during the intervals between successive stimulations, intensity of cold in terms of the 5-point scale, reported thermal indifference at three successive stimulations—the criterion of complete adaptation in this method.

Results

Adaptation. Complete adaptation of the cold spots was reported by all the *O*s in all the experiments and under both methods of stimulation. In the continuous method (results in Table I), adaptation-time averaged 31.7 ± 15.8 sec. for *D*, 49.7 ± 17.6 sec. for *K*, and 20.6 ±9.9 sec. for *L*. As the m.v. (which vary approximately from 35% to 50% of the averages) indicate, the adaptation-times of the individual spots varied considerably: from 9.2—97.4 sec. for *D*, 15.0—80.2 sec. for *K*, and 4.0–41.4 sec. for *L*. The averages, furthermore, were generally shortest

[4] Since complete areal adaptation to cold has never been reported within this range, this temperature variation was of slight consequence.

[5] The length of this interval was based upon the results of Drury and Dallenbach's study (The response of cold spots under successive stimulation, this JOURNAL, 43, 1931, 496).

[6] Except for the substitution of the word "cold" for "warm," the instructions were verbatim as given by Aronoff and Dallenbach, *supra*, 487.

TABLE I

AVERAGE TIME (IN SECONDS) REQUIRED FOR COMPLETE ADAPTATION OF PUNCTIFORM COLD

O		Initial intensity-level of the spots					All of the spots
		1	2	3	4	5	
D	No. spots	0	2	9	8	1	20
	Av.	—	27.7	26.4	41.3	11.2	31.7
	m.v.	—	10.0	10.8	20.2	—	15.8
	Range	—	17.7–37.8	9.2–52.2	14.0–97.4	—	9.2–97.4
	m.v./av.	—	36.1%	40.9%	48.9%	—	49.8%
K	No. spots	0	12	8	0	0	20
	Av.	—	42.2	60.9	—	—	49.7
	m.v.	—	11.7	13.6	—	—	17.6
	Range	—	15.0–77.6	26.8–80.2	—	—	15.0–80.2
	m.v./av.	—	27.7%	22.3%	—	—	35.4%
L	No. spots	3	3	9	5	0	20
	Av.	8.9	15.1	21.3	29.6	—	20.6
	m.v.	2.3	2.5	7.8	10.6	—	9.9
	Range	6.6–12.4	11.4–18.8	12.2–34.0	4.0–41.4	—	4.0–41.4
	m.v./av.	25.8%	16.6%	36.6%	35.5%	—	48.0%

at the lowest intensity-levels (27.7, 42.2, and 8.9 sec. for *D*, *K*, and *L* respectively), and longest at the highest levels (41.3, 60.9, and 29.6 sec. respectively).

Similar results were obtained in the intermittent method (Table II). The number of 1-sec. stimulations (separated by 3-sec. intervals) required for three successive reports of thermal indifference—our criterion of complete adaptation—averaged 36.1 ± 21.7 for *D*, 20.2 ± 6.6 for *K*, and 23.0 ± 12.0 for *L*. As the m.v. again indicate, the number of 1-sec. stimulations required for the different cold spots varied greatly: from 5–206 for *D*, 6–36 for *K*, and 6–68 for *L*. The average times were again smaller at the lower intensity-levels than at the higher levels.

TABLE II

AVERAGE NUMBER OF 1-SEC. STIMULATIONS REQUIRED FOR COMPLETE ADAPTATION OF PUNCTIFORM COLD

O		Initial intensity-level of the spots					All of the spots
		1	2	3	4	5	
D	No. spots	0	3	6	8	3	20
	Av.	—	10.3	27.6	51.7	37.0	36.1
	m.v.	—	3.8	15.3	38.6	6.0	21.7
	Range	—	5–16	8–61	20–206	28–46	5–206
	m.v./av.	—	36.9%	55.5%	74.7%	16.2%	60.1%
K	No. spots	0	9	10	1	0	20
	Av.	—	16.0	22.9	32.0	—	20.2
	m.v.	—	4.9	6.1	—	—	6.6
	Range	—	6–25	12–36	—	—	6–36
	m.v./av.	—	30.6%	22.3%	—	—	32.6%
L	No. spots	0	5	11	4	0	20
	Av.	—	19.6	18.0	41.2	—	23.0
	m.v.	—	11.4	4.7	26.8	—	12.0
	Range	—	6–38	12–26	14–68	—	6–68
	m.v./av.	—	58.2%	26.1%	65.0%	—	52.2%

Counting total time of stimulation, *i.e.* the elapsed time in the continuous method and the sum of the 1-sec. stimulations in the intermittent method, we find that adaptation was on the average slightly shorter in the continuous method for *D* and *L* (4.4 sec. and 2.4 sec. respectively) and much shorter in the intermittent method for *K* (29.7 sec.). On the whole, therefore, there is little to choose, as far as adaptation-time is concerned, between the two methods. This result is surprising as it is contrary to what we expected. We thought that some delay in the time of adaptation time would be occasioned by the intervals of rest in the intermittent method.

As the intensity-levels of the spots used in the two methods (cf. Tables I and II) are comparable, the unexpected result cannot be explained by a difference in the sensitivity of the spots stimulated under the two methods. The stimulus-temperatures, however, are not comparable. Lower temperatures were used with all the *O*s in the intermittent than in the continuous method. The difference was small for *D* and *L* (averaging 2.0° and 4.2°C. respectively) but large for *K* (averaging 6.8°), the very *O* who yielded the smaller average adaptation-time in the intermittent method. Nevertheless, as we shall show below, there is no evidence that adaptation-time and stimulus-temperature are in any way correlated.

Lacking other explanations, we suggest either that the positive after-effect of the cold stimulus is intense and prolonged, or that the recovery period of the cold end-organs is slow. If either or both of these factors are operative, the intervals of rest in the intermittent method would tend to be obscured.

Temporal course. The normal and usual course of adaptation of the cold spots is discontinuous and fluctuating. The intensity pulsates as it fades. It declines gradually but rises suddenly and abruptly. This course was, with a few exceptions, reported by all the *O*s in both methods of stimulation. It was, however, more clearly established in the intermittent method, in which stimulation was discrete and the report specific. The following reports exemplify the pulsating change in intensity-level (0 = thermal indifference, 5 = very intense).

D, (Exp. 5): 3, 1, 0, 0, 1, 0, 0, 1, 0, 0, 0.
K, (Exp. 13): 3, 3, 2, 2, 2, 2, 2, 1, 2, 1, 1, 1, 0, 1, 0, 0, 1, 1, 0, 0, 0.
L, (Exp. 2): 4, 3, 1, 2, 1, 0, 0, 1, 1, 0, 2, 1, 2, 2, 1, 0, 0, 0.

In addition to the normal course of adaptation, one other type was reported: twice by *K* and four times by *L*. In this type, as the following examples show, the cold declines gradually and steadily in intensity from a maximum to complete indifference.

K, (Exp. 2): 2, 2, 2, 1, 1, 1, 0, 0, 0.
L, (Exp. 6): 4, 4, 4, 3, 2, 2, 2, 2, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 0, 0, 0.
(Exp. 15): 3, 2, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 0, 0, 0.

Conditions affecting the time of adaptation. As we have seen, adaptation-time is affected by the sensitivity of the cold spots. Under both methods of stimulation, all our *O*s attain complete adaptation sooner with spots of low-level intensity (cf. Tables I and II) than with high-level spots. As the large m.v. and wide ranges of dispersion at every intensity-level indicate, however, other and more compulsory conditions must be sought. Since the variations in adaptation-time were equally

large during the early and the later parts of a single observational hour and during the early and the later periods of experimentation, practice, fatigue, and ennui may be eliminated from consideration. The effect of stimulus-temperature remains, however, to be examined.

Lower stimulus-temperatures were used with all the *O*s in the intermittent than in the continuous method. Since the differences in adaptation-times under the two methods were not as expected, it is tempting to offer stimulus-difference as an explanation of the discrepancy. A closer examination of our results fails to justify such an explanation.

(1) The temperatures used with *D* were practically constant in all the experiments, varying from 14°–14.5°C. in the continuous method, and from 11°–12° in the intermittent method. The variations in this *O*'s adaptation-times were, never-

TABLE III

AVERAGE TIME IN SECONDS REQUIRED BY THE VARIOUS STIMULUS-TEMPERATURES FOR COMPLETE ADAPTATION

O		Method				
		Continuous		Intermittent		
		11°–12°	15°–17°	8°–9°	9.5°–10.5°	11°–12°
K	No. spots	—	20	9	3	8
	Av.	—	49.7	20.4	19.3	20.5
	m.v.	—	17.6	8.8	4.8	4.7
	range	—	15.0–80.2	6–36	12–23	12–27
	m.v./av.	—	35.4%	42.1%	24.9%	22.9%
L	No. spots	10	10	9	9	2
	Av.	22.7	18.4	26.2	19.8	22.5
	m.v.	12.0	7.0	14.1	11.9	2.5
	range	4.0–41.4	6.6–38.4	13–68	6–68	20–25
	m.v./av.	52.9%	38.0%	53.8%	60.1%	11.1%

theless, larger (m.v./av. = 49.8% under the continuous method and 60.1% under the intermittent) than in those of the other *O*s (*K*, 35.4% and 32.6%; *L*, 48.0% and 52.2%) whose stimulus-temperatures varied from experiment to experiment. If adaptation-time is conditioned by stimulus-temperature, a stimulus of constant temperature should yield smaller variations in adaptation-time than a varying stimulus. While the difference between *D* and the other *O*s is slight, its direction is just the opposite of what this assumption implies.

(2) The discrepancy might, however, if other evidence warranted it, be explained upon the basis of individual differences—but other evidence does not warrant it. *K*'s adaptation-times varied more in the continuous method (35.4%), in which a relatively constant stimulus-temperature (16.5°–17°) was used, than in the intermittent method (32.6%), in which various temperatures (8°–11.5°) were used.

(3) In the case of *L*, temperature varied from one experiment to another in both methods of stimulation. We are consequently able in his case and in *K*'s intermittent experiments to compute the adaptation-times for the various stimulus-temperatures. The results are given in Table III.

K's adaptation-times average longest (20.5 and 20.4 sec.) for high (11°–12°) and low (8°–9°) temperatures. They average shortest (19.3 sec.) for the inter-

mediate range (9.5°–10.5°). *L*'s adaptation-times, on the other hand, average longest (26.2 sec.) for the lowest temperature range, shortest (19.8 sec.) for the intermediate range, and intermediately (22.5 sec.) for the highest range. His averages in the continuous method are longer for the lower range (22.7 sec.) than for the higher range (18.4 sec.). Although we have only a few cases at every temperature range, the conflicting results indicate that in this experiment at least adaptation-time is not conditioned by stimulus-temperature.

In some respects our experiment resembles the liminal experiments with the method of limits. Our continuous method resembles the continuous method of limits, and our intermittent method resembles the serial method of limits. While our physical stimulus was fairly constant, the physiological and psychological conditions affecting *O*'s reports were presumably as variable as in liminal determina-

TABLE IV

AVERAGE TIME IN SECONDS REQUIRED BY THE *O*S FOR COMPLETE ADAPTATION OF PUNCTIFORM WARMTH AND COLD IN BOTH METHODS OF STIMULATION

O	Warmth		Cold	
	Continuous	Intermittent	Continuous	Intermittent
D	12.4 ±4.8	31.0 ±17.0	31.7 ±15.8	36.1 ±21.7
K	13.0 ±9.5	27.8 ±13.3	49.7 ±17.6	20.2 ±6.6
L	24.9 ±13.1	34.5 ±14.6	20.6 ±9.9	23.0 ±12.0

tions. These variable psychophysical conditions may well have a share in determining the time of adaptation.

DISCUSSION

The results of this study with cold spots and of Aronoff and Dallenbach's study with warm spots,[7] are similar. Temperatures that do not yield complete areal adaptation adapt completely when applied punctiformly to warm and to cold spots. While adaptation-times vary with the spot stimulated and with the *O*, nevertheless, as shown in Table IV, they average approximately the same for warmth and cold.

Since the same *O*s served in the two studies, cross-comparisons in adaptation-time are feasible. For *D*, the average adaptation-time of warmth, under both methods of stimulation, is shorter than that of cold; for *L* the relationship is reversed; while *K* has a shorter average for warmth in the continuous method and for cold in the intermittent method. The results of the different *O*s are perfectly balanced. There is little to choose between the adaptation-times of punctiform warmth and cold. Both are, however, much more rapid than the times recently reported for the adaptation of punctiform cutaneous pain.[8]

A short adaptation-time at reported intensities of low initial level holds for both

[7] *Op. cit., supra,* 487ff.

[8] M. Burns and K. M. Dallenbach, The adaptation of cutaneous pain, this JOURNAL, 45, 1933, 113.

warmth and cold, for all the *O*s, and for both methods of stimulation. If level of initial intensity be regarded as a measure of the sensitivity of a spot, then the results of the two studies indicate (1) that adaptation-time of both warm and cold spots is conditioned by the sensitivity of the spots and (2) that, in general, time varies directly with sensitivity: the greater the sensitivity the longer the time required for complete adaptation; and conversely, the lesser the sensitivity the shorter the adaptation-time.

Adaptation-times are shorter under the continuous method for all the *O*s (18.6, 14.8, 9.6 sec.) with warmth, and for *D* (4.4 sec.) and *L* (2.4 sec.) with cold. For *K*, on the contrary, adaptation-time with cold is shorter (29.7 sec.) under intermittent stimulation. The intervals of rest between successive stimulations in the intermittent method, by permitting partial recovery, should have delayed adaptation. Since adaptation was not long delayed, it seems that the positive after-effect of the stimulus, or the slow recovery of the end-organs, or both together, obliterated the effect of the intervals of rest. The delay of adaptation under the intermittent method was, however, greater for warmth than for cold. Indeed, *K* showed no delay at all in the case of cold—he adapted more slowly under continuous stimulation—and the delays for *D* and *L* were slight. These results suggest that after-effect of stimulation is more pronounced in the case of cold than in the case of warmth.

The normal course of adaptation is the same for the warm and the cold spots: a pulsating decline in which intensity fades gradually and rises suddenly several times before thermal indifference prevails. This course resembles closely a variant type of pain adaptation reported by Burns and Dallenbach,[9] and the "multimaximal type" of pain adaptation reported by Wells and Hoisington.[10] For the warm spots there are no variations from the normal course of adaptation, but for the cold spots, one other type was reported. Two of the *O*s (*K* and *L*) reported six instances of cold simply becoming weaker and weaker and fading out—a course that resembles the normal course of pain adaptation,[11] especially the non-pressury type of pain adaptation reported by Wells and Hoisington.[12] The normal in temperature resembles the variant in pain, and the variant in temperature resembles the normal in pain.

Adaptation-time of both warm and cold spots are conditioned by their sensitivity. Spots of low intensity-level adapt in shorter time. Large m.v. and wide ranges of dispersion at every intensity-level in both warm and cold spots imply the presence of additional factors. In both studies practice, fatigue, and ennui were ineffective.

The stimulus-temperatures were constant in Aronoff and Dallenbach's study with the warm spots,[13] but in the present study different temperatures were used. There is no indication, however, that thermal adaptation is conditioned by the temperature of the stimulator.

Summary

The results of this study may be summarized as follows:

(1) Cold spots, like the warm spots, adapt quickly and completely under puncti-

[9] *Op. cit.*, 116.

[10] E. F. Wells and L. B. Hoisington, Pain adaptation: A contribution to the Von Frey-Goldscheider controversy, *J. Gen. Psychol.*, 5, 1931, 359 f.

[11] Burns and Dallenbach, *op. cit.*, 115.

[12] *Op. cit.*, 361.

[13] *Supra*, 486.

form stimulation—whether continuously or intermittently applied—at temperatures inadequate to areal adaptation.

(2) The normal course of cold adaptation is, like that of warm adaptation, discontinuous and fluctuating. Intensity fades gradually and rises abruptly several times before thermal indifference prevails. In addition, the cold spots yield a variant type in which intensity declines gradually and steadily and simply fades out.

(3) Adaptation of cold, like that of warmth, is rapid. The time, in both alike, varies with the sensitivity of the spot, with the *O*, and with the method of stimulation. There is no indication, however, that adaptation-time is in any way conditioned by stimulus-temperature.

(4) The after-effects of stimulation are even more prolonged in cold than in warm spots.

APPARATUS

A WAX-PAPER KYMOGRAPH

By RALPH GERBRANDS and JOHN VOLKMANN, Harvard University

Red or blue paper covered with a thin coating of white wax is now often used as a medium for graphic recording. As Krueger has pointed out,[1] the mechanical energy needed to make a record on this paper is considerably reduced if the stylus is slightly heated. The advantage gained in this way is so great that, for most psychological applications, the wax-paper kymograph can replace the smoked-paper kymograph. The instrument to be described secures the obvious advantages of wax-paper recording over smoked-paper recording: there is no preparation of the paper before recording, and no fixing afterward; long records are as convenient to make as short ones; there is much less danger of smudging the record; the whole process is cleanly. The design of the instrument provides additional advantages: a fairly wide range of paper-speeds; constancy of any given speed; an instrument-support rigidly mounted on the kymograph; convenient control of heater-temperature. The instrument operates in both vertical and horizontal positions and serves as a kymograph and polygraph. The design employs Krueger's third method of recording: a long stationary heater radiates heat to a light metal stylus which conducts part of this heat to the paper; the stylus then leaves a narrow line of melted wax through which the color of the paper shows plainly.

Numbers in the following description refer to the accompanying phantom figure (Fig. 1). The frame of the machine is composed of three steel plates (1, 2, 3), each 7 in. × 8 in. × 3/16 in., separated by eight steel spacers (4). The middle plate (2) divides the machine into two compartments. The upper compartment houses the roll of wax-paper, the stylus-heater and driving rolls; the lower compartment houses the motor, its mounting and the electrical controls. The inside height of the lower compartment is 5 in., and of the upper, 6 1/16 in.

The paper (5) is 6 in. wide and is available in rolls 300 ft. long.[2] It is drawn with shaded edges in the diagram. The roll has a 1-in. paper core; inside the core is placed a 1-in. brass tube (not shown), into which is telescoped a smaller tube. A spring contained within it forces the end of the smaller tube against the bottom of plate (1); this device serves as a brake to keep the paper taut in feeding. The paper and core-assembly are held in place by a steel rod (6), which is removable from the top of the machine. The paper feeds off the roll, over a small guide-roll (7), over the writing-plate (8), and over a guide-roll (9). The paper then runs across the front of the machine to the guide-roll (10), and to the driving unit. This consists of corrugated steel roll (11), and a rubber roll (12) pressed against (11) by a spring. The paper feeds directly out through the side of the machine. Rolls

[1] R. G. Krueger, An improved system of kymographic recording, *J. Exper. Psychol.*, 14, 1931, 176-179.

[2] The paper may be obtained from the Stylograph Corporation, Coldwater, New York, at a cost to educational institutions of approximately $0.95 per roll.

(7) and (9) are made of ¼-in. drill-rod. Plate (8) is a steel strip ¾ in. wide. Roll (11) and its shaft are turned to 1 3/16 in. diameter from solid steel; the corrugations are formed with a heavy knurling tool. The roll (12) is made of a steel core and a rubber jacket for a 1¼-in. typewriter platen. This roll is likewise turned to 1 3/16 in. because it is to be geared to (11).

The motor[3] (13) is a 1/75 H.P. synchronous motor for use on 110 volts A.C. lines. It runs at 1800 R.P.M. and is equipped with self-contained double worm-gear

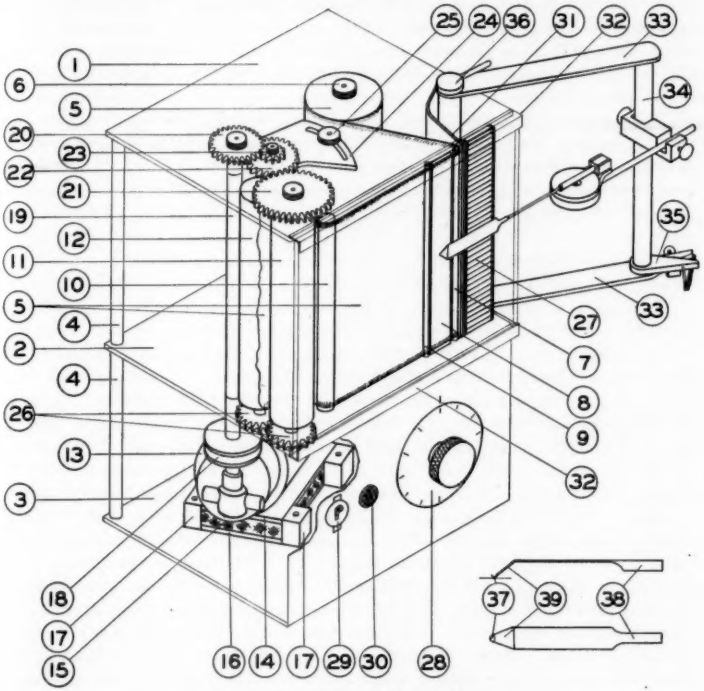


FIG. 1. WAX-PAPER KYMOGRAPH

Phantom view. See text for description. Detail of stylus in lower right corner.

reducer with a ratio of 510:1. The motor (13) is bolted to a piece of sole-leather (14); this is firmly glued to sponge-rubber (15); this is glued in turn to sole-leather (16), which is bolted to plate (3). This mounting insulates the kymograph against the vibration of the motor. Wooden blocks (17) prevent excessive horizontal movement of the motor under the torque of the load at high paper-speeds; the blocks also aid in supporting the motor when the machine is recording in the hori-

[3] Obtainable from the Bodine Electric Co., Chicago, Illinois.

zontal position. The torque of the motor is transmitted through a flexible coupling (18) which consists of two brass disks 2¼ in. in diameter, and a leather disk between them. Pins in the brass disks engage with holes punched in the leather disk. The drive-shaft (19) is 5/16 in. in diameter and runs in brass bushings pressed into plate (1) and (2). The gear (20) slips on the keyed end of the shaft (19). The driven gear (21) is similarly fastened to the shaft of roll (11). The idler-gear (22) turns freely on a movable stud (23); this stud slides in a slot in gear-yoke (24). The slot (not shown) permits the meshing of gears of different sizes in positions (20) and (22). A yoke (24) rotates as a whole about shaft (19), so that the gear (22) can be made to mesh with the gear (21). The yoke is clamped fast at stud (25). Gears (20) and (21), and stud (23), are fastened down by thumb-nuts as shown.

The gears are 32-pitch, in stock sizes; the sizes are indicated by the number of teeth as follows: (one each) 18, 24, 32, 64, 72; (two) 48; one compound gear, consisting of a 16-tooth and a 48-tooth gear fastened together. Either the compound gear or a single idler gear may be used in position (22). Gears may be inverted to fit with the compound gear. Different combinations of the gears provide fourteen paper-speeds, from 1.08 in./sec. to 58.5 in./sec. Recording is possible at speeds both higher and lower than this range, but such speeds require a different design. Speeds which are too high overstrain the coupling and motor-mounting.

The design contains four features which keep the paper running properly, without slipping or running off at the side. (a) The distance from the paper-supply to the driving-rolls is relatively short—approximately 10 in. (b) Accurate alignment of all rolls is secured by drilling plates (1) and (2) together. (c) Driving rolls (11) and (12) are geared together at the bottom by gears (26); these are 38-tooth, 32-pitch gears cut to order. (d) The pressure between rolls (11) and (12) is equalized along the length of these rolls. A spring of adjustable tension bears against the middle of a rod; the rod is pivoted at its ends to two movable arms which form the bearings for roll (12). These parts could not be shown in the diagram.

The heater (27) is made of No. 32 Nichrome V resistance wire, 12 ft. long and wound in 120 turns; its resistance is 90 ohms, and the maximum current, at 115 volts, is 1.2 amp. (These values are approximate.) The wire is wound on a mica strip ½ in. wide. An asbestos-lined metal channel holds the wire-wound strip. The strip is fastened in the middle only, to permit its expansion upon being heated. Heat rises in the chamber formed by the heater and the glass front of the machine. If the kymograph is to be operated most frequently in the vertical position, the mica strip may be made slightly wider at the bottom, to provide additional heat for recording at the bottom of the paper. The amount of heat needed for recording increases with the speed of the paper and also with the speed of the stylus as it moves transversely. Too much heat produces a wide line on the paper, and too little heat, no line at all; fortunately, the adjustment is not critical. When the stylus-movements are rapid but infrequent, more heat is needed, and the line is consequently wide between movements. Variable auto-transformer[4] (28) controls, within the range 0-115 volts A.C., the voltage applied to the heater, and hence the amount of heat radiated. Motor-switch (29) and pilot-light (30) complete the electrical controls.

[4] "Variac" type 200-B, manufactured by the General Radio Co., Cambridge, Mass.

The wax-paper runs close to the heater in order to reduce the length of the stylus and the heat lost in conduction through it, but the paper must also be insulated against direct melting of the wax by the heater. This insulation is accomplished by a radiating fin (31). The stylus must be protected from drafts of air; this is done by a glass plate which slides in grooves (32) and covers the upper compartment of the instrument in front. This plate and the back of the kymograph are removable to permit loading with new paper. Rubber feet and firmly fastened side-plates guard against excessive vibration and noise.

The instrument-support holds the recording instruments rigidly, and keeps them parallel to the writing surface. This support consists of two parallel steel arms (33) which are rigidly connected at the outer end by a rod (not shown). This rod serves as a bearing for brass tube (34), ½ in. outside diameter. The recording instruments are clamped to this tube as shown. The tube (34) may be rotated through a small angle by an adjustment (35), in order to move all the styli simultaneously toward the paper or away from it. The whole instrument-support may be rotated about its inner end to the optimum position; it is then held there by clamp (36). When the machine is used as a polygraph, the support will hold seven pencil-type signal-magnets in small clamps.

The metal stylus, shown in detail, is made of aluminum sheet 0.002 in. thick, approximately ¼ in. wide, and 2 in. long overall. The stylus operates parallel to the heater and approximately ¼ in. in front of it. The recording end (39) is blunt in order to allow maximum conduction of heat to the paper. The point (37) is a hemispherical indentation, 1/32 in. in diameter made with a round-pointed punch; this feature provides a small smooth surface which traverses the wax with minimum friction. The shank (38) is formed into a tube to receive the writing-straw. The flat inside surface of the stylus is coated lightly with flat black baking enamel; the coating increases the amount of heat which the stylus absorbs, and consequently increases the efficiency of recording.

The parts of the machine cost $55.00, and the construction requires approximately 100 hours of labor. Such a machine has been used in the Harvard Laboratory to record breathing, pulse, writing-pressure, speed of tapping, and signals from a problem-box.

PROJECTION TACHISTOSCOPY

By SIDNEY M. NEWHALL, Johns Hopkins University

Several simple methods of securing controlled exposures with projection lanterns may not have occurred to some who would find them useful. The combined or so-called universal projector which takes either lantern slides or opaque material may be very easily adapted to serve as a tachistoscope. The desired slide is inserted as usual and the exposure is defined by a sector disk rotating at constant speed just in front of the projecting lens barrel. Before exposure the lantern is set for opaque projection with the preëxposure material in the opaque holder. When the operator wishes to make an exposure he moves the lever which adjusts the lantern for slide projection and after the open sector has passed the lens, he shifts the

lever back. Experience shows that with a disk revolving as rapidly as 1 r.p.sec. and with an open sector as wide as 60°, there remains sufficient time to make an inaudible shift from opaque to slide projection and back again.

If the tachistoscope is used in a dark room the subject sees in order: the illuminated preëxposure field, darkness, the exposure field, darkness, the post-exposure field. The two brief dark periods are neither ideal nor necessarily disturbing. They can scarcely be expected to grossly affect fixation or level of light adaptation both

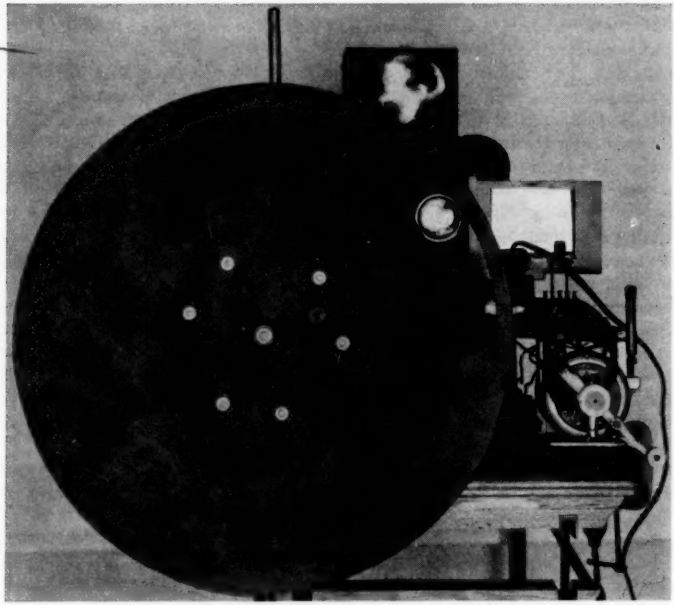


FIG. 1. A PROJECTION-TACHISTOSCOPE

of which are maintained by the relatively long pre- and post-exposure fields. The dark periods can be completely eliminated, however, by operating the device with the opaque projection lens capped and with continuous, moderate illumination falling on the screen from an independent source. The latter could be a second lantern as in the Twitmyer tachistoscope,[1] but the mere employment of suitable room illumination avoids any such complication. Indeed, much slide material is highly visible with moderate general illumination and the latter has the advantage of facilitating the recording of responses by the *S*s. On the other hand, the fact that

[1] E. B. Twitmyer and S. W. Fernberger, Some new laboratory and demonstrational apparatus, this JOURNAL, 38, 1927, 113-116.

the extraneous light falls on the screen even during the exposure proper is theoretically, at least, a disadvantage. Also, there is now no satisfactory way of providing patterns in the preëxposure field.

A useful compromise procedure is to operate in the lighted room with the opaque projection lens uncapped. While this does not eliminate the theoretical disadvantage, disturbing contrasts may be avoided and at the same time desired preëxposure material, such as a fixation-mark, can be projected.

Fig. 1 represents a model, utilizing the Spencer Delineascope V A, which was assembled in about two hours from parts available in almost any laboratory. The 19 in. disk was cut from heavy cardboard, painted black on both sides, and attached by several screws to an 8-in. drive pulley. The free axis of the pulley is clamped to a laboratory stand in such a position that the aperture will register with the slide projection lens while the periphery of the disk will not obstruct the light from the opaque projection lens. The aperture is a simple cut-out in the form of an annular segment which is just large enough to permit full exposure during 36° of rotation. Therefore, when running at one revolution per second, the duration of exposure is 0.1 sec. or 100 σ. The disk is driven with a thread belt by the adjustable speed dictaphone motor shown at the right of the figure.

Time calibration is quickly accomplished or checked by timing the disk with a stop-watch to determine the duration in seconds for the single revolution. This value multiplied by the fractional width of the open sector is the duration of exposure. For most purposes it seems well to define the exposure as the period during which the field is completely exposed. For instance, if a complete exposure for 0.1 sec. is desired at 1 r.p.sec., the width of the annular segment must be 36° plus approximately the diameter of the projecting lens. Measuring the diameter of the lens is a simple matter and this easy adjustment is probably satisfactory for most purposes. Involved is the assumption that the lens is identical with or but slightly larger than the exit pupil of the lantern, but that is regularly the case in efficient design.

Brightness calibration consists in measuring field brightness with a portable photometer directed at the screen under the illumination conditions of the experiment. Naturally, the results will vary with the reflectance of the particular exposure materials employed.

A variable speed motor allows great change of exposure without changing disks, but there is also a change in the rate of cut-off. For durations of the order of 100 σ and an aperture 8 in. from the axis of rotation, the exposure appears instantaneously complete and variation in onset or offset may not be considered to be a source of error. If it is, the speed of cut-off may be held constant by running at constant speed and the duration varied by using disks with apertures of varied width but at uniform distance from the axis.

A problem or differentiating characteristic in disk tachistoscopes is the device employed to confine the exposure to the desired revolution. One way of doing this is to shift from opaque to slide projection and back, as already described. Another way, which has the advantage of applying to the simple lantern as well as to the combined instrument, is to move the slide into position before and out of position after the open sector has passed by. This procedure requires an opaque rectangle of slide size in the other half of the slide holder to prevent periodic flashing between desired exposures. The holder can be moved into position without much

rattling and brightness of the pre- and post-exposure fields can be controlled by roomlight.

A factor which has retarded the development of projection tachistoscopy is the expense of the usual commercially prepared slides. In this connection it may be recalled that much slide material can be quite readily prepared by the experimenter himself in accordance with the helpful suggestions of Schlosberg.[3] Still, in exploratory work involving trial of numerous variations, opaque materials may prove to be easier and less inhibiting. Under such circumstances the combined lantern can be made to serve for opaque projection in the main exposure. This is done by arranging the time control device to expose the opaque projection lens without intercepting light from the slide projection lens which is now to supply the pre- and post-exposure fields. While the brightness of those fields can be regulated by inserting slides of appropriate density, it should be noted that the maximum intensity of the exposure field will be relatively low and so insufficient for some purposes. The dark intervals preceding and succeeding exposure can be eliminated and the contrasts can be reduced by methods mentioned above. Because of the size of disk necessary to secure a sharply defined exposure with the large aperture lens, a pendulum or fall mechanism may be preferred.

Relatively long exposures, of a number of seconds, are easy to control because the absolute error may be relatively large and no disk or other accessory is required for beginning and terminating the exposure. The operator merely shifts from opaque to slide projection and back, or vice-versa; or the slide itself may be moved into and out of position. If a sector disk has been mounted, there is the third expedient of turning the aperture into and out of position by hand. Exposure by such methods can be gauged with a watch or by counting the beat of a seconds pendulum. By practice with the latter the variation of the single exposure can be reduced to 0.5 sec. or less. For durations around 10 sec. this means a *range* of relative error of only 5%.

None of the methods described conforms exactly to all of the traditional requirements of the ideal exposure-instrument, but this is scarcely an objection to one who accepts the principle that a simple technique which affords sufficient control for the purpose is more scientific than an unnecessarily elaborate one.

SOME GUSTATORY APPARATUS

By KARL M. DALLENBACH, Cornell University

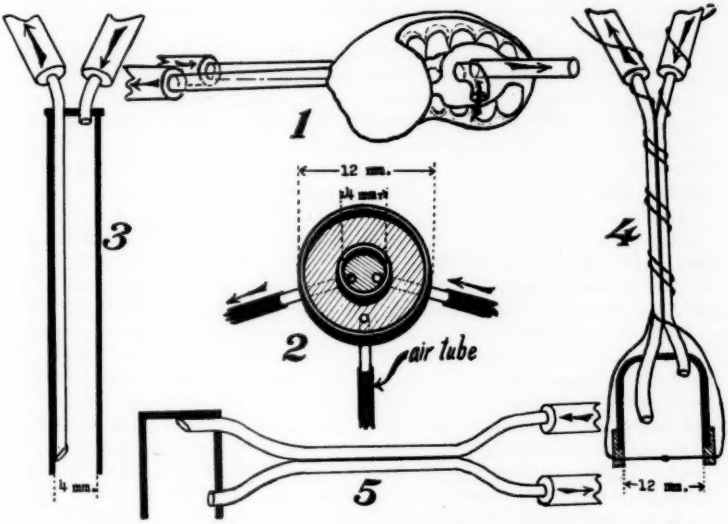
The devices shown in the accompanying diagrams (Figs. 1-5) have been constructed during the past few years in the Cornell laboratory for experiments in gustation.

Mouth-piece. The mouth-piece shown in Fig. 1 was devised especially for studying taste adaptation but it is also suitable for determining the gustatory limens. It consists of two glass tubes inserted in dental impression compound—a thermo-

[3] H. Schlosberg, A projection tachistoscope, this JOURNAL, 43, 1931, 501.

plastic wax rigid at mouth temperature but softening at a slightly higher temperature (*i.e.* 40°-45°C.).

To make the mouth-piece, soften a cake of dental impression compound in a waterbath and then form it into a double conical roll—bases together, apexes apart. Push two pointed glass rods through the roll near its center and bend the roll into horseshoe shape to conform to the dental arch of the *O* for whom the mouth-piece is being prepared. Have *O* bite into the compound, bringing his incisors



FIGS. 1-5. GUSTATORY AND THERMAL STIMULATORS

down upon the glass rods, and mold the compound on the outside of his teeth with his lips and fingers, and on the inside with his tongue. When the compound has hardened, remove and place it under running cold water for further hardening. Remove the glass rods after gentle heating, and replace them with glass tubes, one (the inlet tube) projecting straight into the back of the mouth, the other (the outlet tube) curving to end in the sublingual region just back of the lower incisors. After adjusting the tubes to a comfortable length in the mouth, apply a heated awl to the compound immediately around the tubes to seal them firmly in position.

When the mouth-piece is attached to conducting tubes and placed in the mouth, the stimulus-solution is carried to the back of the buccal cavity and from there it flows forward along the sides of the tongue and passes out of the mouth from a point in front. If the rate of flow is kept constant by means of Mariotte's bottles[1]

[1] E. L. McCarthy, Mariotte's bottle, *Science,* 80, 1934 (no. 2065), 100.

or other mechanical devices,[2] the stimulus-solution will maintain the constant pressure and temperature required in the study of gustatory and thermal sensitivity or adaptability of the membranes in the mouth cavity.

Punctiform stimulators. Because the mouth-piece does not permit the control of stimulus-area, the instruments shown in Figs. 2 and 3 were devised to confine the stimulus-solution to a relatively small area—possibly to a single papilla. Neither of the instruments, it should be said at the outset, proved wholly adequate for gustatory stimulation, but their inadequacy, it should also be said, could not have been foreseen.

The instrument shown in Fig. 2, an adaptation of Lashley's saliometer,[3] was made first. It consists of two concentric circular chambers, 4 and 12 mm. in diam., respectively, and 3 mm. deep. Two tubes leading to the inner (stimulus) chamber, serve as inlet and outlet for the stimulus-solutions, and one tube leading from the outer chamber is connected by a stiff-walled rubber tube to a suction pump. When placed on the tongue the instrument clings to it as soon as the air is exhausted and a vacuum is formed in the outer chamber.

Our idea in devising the instrument was to place the inner chamber over an isolated taste papilla, to hold it fast in position by means of the suction of the outer ring, and then to stimulate the area covered by the inner chamber with various taste-solutions.[4] Theoretically the instrument should have worked, but in actual practice it failed because of involuntary movements of *O*'s tongue and because of the pressure of the solution in the inner chamber. These factors singly or in combination invariably break the seal of the outer ring and the instrument falls off the tongue. The instrument, however, clings tightly to the mucous membrane of the cheek, and so it may prove useful in experimenting with that part of the buccal cavity. It cannot, however, be used on the tongue unless it is held in position. If it has to be held in position, there is no need for the outer chamber nor for keeping the inner chamber short in length or light in weight—desiderata when the instrument is to be held in place by suction.

In the next instrument made, Fig. 3, the outer chamber is eliminated and the inner chamber extended to form a handle. This stimulator, a copper tube 12 cm. long and 4 mm. in diam., is provided with an inlet near the top and an outlet near the bottom. The open end of the stimulus-tube is placed over a functional taste papilla,[5] and the instrument is held in position by *O* while *E* introduces the stimulus-solution into the chamber. In principle, again, this instrument should have

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[2] J. H. Wales, Device for constant flow of liquids, *ibid.*, 79, 1934 (no. 2059), 545 f.

[3] K. S. Lashley, Reflex secretion of the human parotid gland, *J. Exper. Psychol.*, 1, 1916, 462 f. Cf. also J. B. Watson, The place of the conditioned reflex in psychology, *Psychol. Rev.*, 23, 1916, 91 f.

[4] The solution, isolated within the inner chamber, cannot be diluted with saliva. Only when the experiment requires a constant temperature, therefore, above or below physiological zero is it necessary to circulate the solution through the instrument. If it is not circulated, its temperature within the inner chamber will, because of the heat of the tongue and of the small amount of solution involved (about 1/26 cc.), quickly reach physiological zero.

[5] The papillae were first tested for sensitivity with a camel's hair brush that had been dipped into the stimulus-solution.

worked, but in actual use no taste experience emerges even when concentrated solutions were introduced into the stimulus-chamber.

The aperture of the stimulus-chamber is small (4 mm. in diam.), and the instrument must be held against the tongue with considerable pressure in order to prevent leakage, consequently a steep pressure gradient is formed. Since the tissues of the tongue are soft, the pressure gradient formed may suffice to cause a pressure block in the sensory nerves leading from the papilla stimulated. Whatever the final explanation of the failure of this instrument may be, the possibility of limiting the stimulus-solution to a single papilla by means of small stimulus-apertures had to be abandoned.

Areal stimulators. The areal stimulators that were made next are shown in Figs. 4 and 5. The stimulus-chambers of these instruments are 12 mm. in diam., and 15 mm. in height. Two brass tubes, inlet and outlet, lead respectively to the top and bottom of the chambers. They are bent to the form of a cantilever and soldered together into a rigid handle. In the first of these instruments (Fig. 4), designed for stimulating the protruded tongue, the tubes lead from the top of the stimulus-chamber, and in the other instrument (Fig. 5), designed for stimulating the back of the tongue, they lead from one side.[6]

These stimulators are effective when they are applied to areas near the edge of the tongue and the stimulus-solutions are circulated through them.[7] Apparently, the pressure gradient—if that be the cause of our failure with the smaller stimulators—is not sufficiently extended to block the nerve-fibers from the papillae in the center of the area stimulated, though blocking may have occurred near the edges of the stimulus-chamber.

To determine and control stimulus-temperature, removable iron-constantan thermocouples are attached to the stimulus-chambers of these instruments.[8] The thermocouple junction is placed at the center of a hard rubber ring milled to fit over the end of the stimulus-chamber in a waterproof joint. Fig. 4 shows the ring—the cross-hatched extension to the stimulus-chamber—and the thermocouple in place. The lead wires are wrapped around the handle (the inlet and outlet tubes) of the instrument, and are carried to the galvanometer and constant-temperature bath.[9]

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[6] This instrument, we discovered later, is similar to the glass stimulator devised by Hahn and Günther (cf. Helmut Hahn and Herbert Günther, Über die Reize und die Reizbedingungen des Geschmackssinnes, *Arch. f. d. ges. Physiol.*, 231, 1932, 49 f.) for studying the effect of stimulus-temperature upon gustatory sensitivity. The stimulus-chamber of their instrument is, however, slightly larger, being 15 mm. in diam.

[7] The diameter (12 mm.) of the chambers was the only one tried after our failure with 4 mm. Since we were seeking an instrument with which we could study gustatory adaptation when stimulation was restricted to the tongue, we were content as soon as we had obtained one. We did not turn from our object to pursue the interesting problem of the minimal diameter of an areal taste stimulus. We hope, however, to turn to it at some future time.

[8] In Pfaffmann's modification of the Hahn-Günther apparatus (Carl Pfaffmann, Apparatus and technique for gustatory experimentation, *J. Gen. Psychol.*, 12, 1935, 446-447), a similar control is added. He fused a thermocouple junction of chromel-alumel in the stimulus-chamber, which in his instrument is made of glass.

[9] For a description of the apparatus and method of measuring the temperature by means of the galvanometer and thermocouple see Thomas Lewis, *The Blood Vessels of the Human Skin and Their Responses*, 1927, 11-16.

APPARATUS NOTES

A Modification of Greenwald's Apparatus for Measuring Electrodermal Responses

An electrical circuit using direct current for measuring the electrodermal response similar to one previously published is described in this paper.[1] This modification is especially adapted for use where a constant supply of 110 v. 60~ current is unattainable or where an extremely stable and precise measuring instrument is desired. It differs from the circuit previously described by using batteries as the source of exosomatic current rather than 60~ current, and in the elimination of the copper oxide rectifiers. Since the current through *S* is direct, simple plate electrodes cannot be used without introducing error and some form of 'non-polarizing' electrode must be substituted.

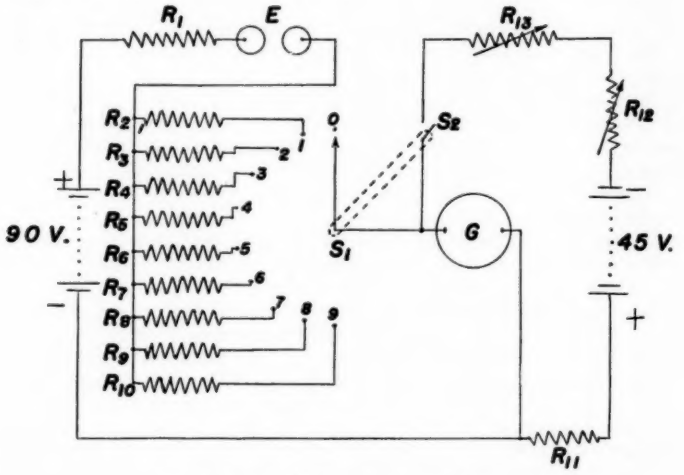

Fig. 1. Circuit Diagram of the Apparatus

E = Electrodes	$R_3 = 120{,}000$	$R_9 = 30{,}000$
S_1, S_2 = Switches	$R_4 = 105{,}000$	$R_{10} = 15{,}000$
G = Galvanometer	$R_5 = 90{,}000$	$R_{11} = 400{,}000$
$R_1 = 850{,}000$	$R_6 = 75{,}000$	$R_{12} = 100{,}000$
$R_2 = 135{,}000$	$R_7 = 60{,}000$	$R_{13} = 10{,}000$
	$R_8 = 45{,}000$	

The battery supply consists of three 45 v. radio 'B' batteries. Precision wire wound resistors of the values given are used in all cases save the variables R_{12} and R_{13} which are wire-wound radio potentiometers. S_1 and S_2 operate together and involve the use of a 10-point two-gang switch. To form S_2 all poles in one gang, save one

[1] D. U. Greenwald, An apparatus for measuring the electrodermal response, this Journal, 47, 1935, 682-685.

for the 'off' position, are shorted together. The above circuit values were determined to match a galvanometer having a figure of merit of 0.025 μa./mm. scale deflection. Galvanometers of other sensitivities may be used if the values of R_1 and R_{11} are properly varied.

The current through S is 0.09 mils. when his resistance is 15,000Ω or any multiple of this value. In order to keep the galvanometer within reading range this measuring current is balanced by means of a 45 v. potential through a 0.5 meg. resistance composed of R_{11}, R_{12}, and R_{13}. If S's resistance drops 15,000Ω the measuring current increases by 1.573μa.; a change of almost 2%. This is the maximum current variation possible since a further drop in resistance necessitates the use of the next lower tap on S_1. Such a maximum current change will swing a galvanometer with the above sensitivity 62 scale units.

The circuit is balanced by shorting the electrodes (E), placing S_1 on the first tap, and adjusting R_{12} and R_{13} until the galvanometer swings to a 'zero' point on one side of the scale. This point is so selected that its value is one half that of a full scale deflection (in this case 31 units). The insertion of a 15,000Ω resistance at E will then cause the galvanometer to swing an equal distance to the other side of its rest position. With the circuit balanced the galvanometer swings may be read as resistance values by a calibration of the scale against a resistance box inserted at E. Thus with S_1 on tap 1 resistances from 0 to 15,000Ω may be read. On tap 2 resistances from 15,000Ω to 30,000Ω appear, and so on up to 150,000Ω on tap 9. With the galvanometer mentioned above a resistance change of 250Ω would cause a deflection of approximately one scale unit.

Due to the resistance variation of different types of copper oxide rectifiers considerable experimentation is involved in setting up the alternating current circuit previously published. This difficulty should not obtain in the circuit described.

University of Iowa D. ULRICH GREENWALD

THE "YOGI PUZZLE" AND THE "ENDLESS SPIRAL": DEMONSTRATIONAL DEVICES OF APPARENT MOVEMENT

The "Yogi Puzzle" and the "Endless Spiral," on sale at many stores, depend upon and beautifully illustrate some of the principles and types of apparent movement. A record and a description of these novelties may, therefore, be of interest to psychologists.

In the "Yogi Puzzle," a sword is seen to pass through the neck of a girl without beheading her.[1] In the operation of the puzzle (Fig. 1), a head-shaped metal pin (A) is inserted in a hole (B) at the headless shoulders of the girl prone before a yogi. The pin, when inserted, extends entirely through the device. Directly above the shank of the pin, which represents the girl's neck, the yogi's sword is suspended. When the lever arm (C) is pushed down, the sword passes from its position above the girl's head to one immediately below it; when the lever arm is pushed up again, the sword passes back to its original position. As the lever is pushed back and forth, the sword flicks up and down, passing every time apparently without hindrance through the solid steel 'neck.' The illusion is compulsory and

[1] This puzzle may be obtained from its distributors, The Franco-American Novelty Co., 1209 Broadway, New York, N.Y.

complete. Many people who are familiar with the principles of phenomenal movement are baffled by this device and are at a loss for an explanation of the mechanical basis of the illusion.

The explanation is simple. The device is constructed on the principle of an electrical toggle switch. When the lever arm (C) is moved, a spring is compressed and suddenly released, moving the 'sword' arm rapidly through the angle *X* (see insert in Fig. 1) at a rate exceeding the limits of perception. The result is apparent

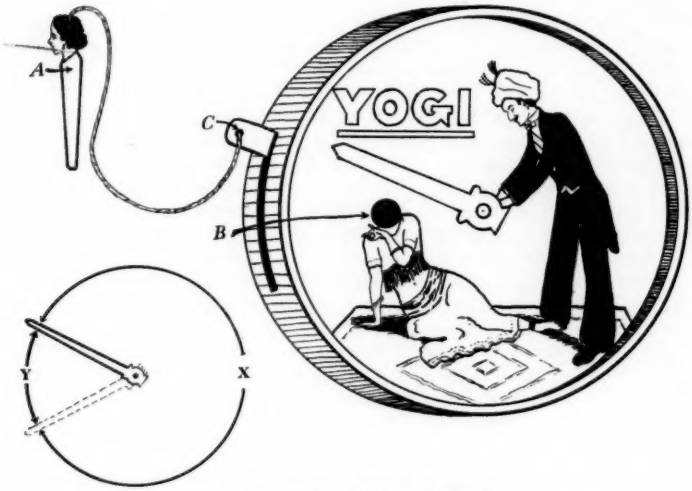

FIG. 1. THE "YOGI PUZZLE"

movement through the angle *Y*. Knowledge fails to affect the illusion, and it is as striking for the initiated as for the novice.

The puzzle is about the size of a common watch. It is suitable for individual demonstration, but not for classroom use. We are hoping to construct a large model for group demonstrations. The puzzle may in its present size be shown to a class by means of a reflectoscope, but then the illusion suffers by loss of objectivity.

The "Endless Spiral," two spiraling double wires entwined for a short length and firmly fastened together (Fig. 2), gives when properly manipulated an illusion of indefinite expansion or of indefinite contraction. If one grasps the wires loosely with the two hands near the middle concealing the ends within the palms, and draws the hands slowly apart, allowing the fingers to slip along the spiral and the wires to revolve, the spiral seems to unwind and to expand endlessly in length. If the hands are placed at the ends and the fingers are slipped along toward the center, the opposite effect—an endless contraction—is realized. The illusion in either case

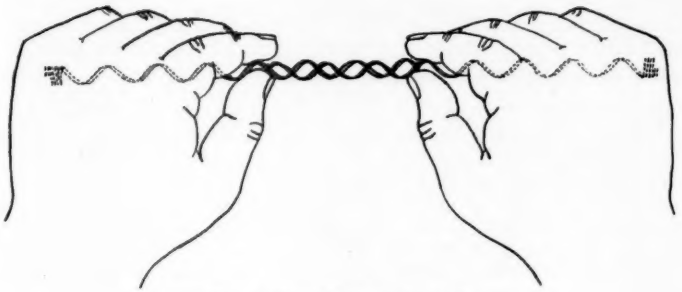

FIG. 2. THE "ENDLESS SPIRAL"

is, again, complete and compulsory, but it is enhanced by repetition and by alternating the outward and inward movements of the hands. In the last case, the wires seem to grow and to contract alternately.

Cornell University KARL M. DALLENBACH

NOTES AND DISCUSSIONS

Conjunctive Research in the Sciences of Life

Is it not presumptuous to extol at Yale University the virtues and the benefits of coöperation?[1] One does not carry coals to Scranton, exhort Philadelphians to love their brethren, present treatises upon the chimpanzee to Yerkes, or instruct Gesell upon the most adroit method of filming the man-baby. Should an outsider burrow into this Woods Hole of snug amity to advertise a new device for rocking the seminary table, all ringed 'round with overlapping hands and magnetic from common zeal and a single purpose? Here where—as among Margaret Mead's antipodal Arapesh—each man plants his bean or his squash that it may be avidly devoured by others, not by the tongue of selfishness. Each for other! One for all!

Speaking in any other place and under other auspices, my subject would run the risk of exciting contrary opinion and clashing prejudice; for our guild is full of partisans for or against coöperation, pooling, and conjunction. Most comments upon this timely topic are therefore fixed opinions either *pro* or *con;* opinions as cemental as those voiced for or against the ethics of permitting a Chief Executive to serve his beloved friends and neighbors for a second official term.

Upon you I can count to sustain the great pro-coöperationist cause. You are this evening guests of The Institute, a great *familia,* and moreover you are—as I may hope—looking forward to a home-bound passage with travel-expense coöperatively provided. Were I in need of a further pledge of your friendly attitude toward my subject, I should find it in your common allegiance to the Great National Research Coöperative, which is sustained by the principle of conjoint contribution to science, and which is the constant scene of busy little congregates continually composing and decomposing under that powerful catalytic agent, the scientific conference.

Your coveted approval given, you may nevertheless—when the expense voucher has been properly executed—qualify your conviction. "Yes," you may cautiously agree, "coöperation of a sort and in its proper place. Of course, nothing takes the place of the scientific hermit in his cell, alone with his god and his apparatus. But the hermit may be permitted to venture out with the ground-hog after the winter solstice, graciously accept a ticket to Washington (or New Haven), and then modestly hold aloft his little candle to lighten his fellows." Inspiration, insight, and discovery really demand a dirt-floor and chillblains, but after all the light of science must shine, especially at Candlemas. The great men must convene, read and discuss papers, join the Babel of tongues, cement organizations, and modestly capitulate to interviews with The Press.

Coöperation 'of a sort and in its proper place!' That concession seems to offer food for thought. "What sort?" we may go on to ask. "And what *is* the proper place?"

Our commonest instrument has been the conference—conference of men on a common subject working with a common method on a common topic; men converging toward a common problem by a variety of methods; men in related fields seeking to cement their results to one whole, and so on. In an idle hour I once made a list of species and varieties of conference, distinguishing them by aim and

[1] An address delivered February 7, 1936 before the Society for Research in Child Development at the Institute of Human Relations, Yale University.

by function. When I had counted seven sorts, I became suspicious that I was only setting up a little finite series within an infinite order. Probably the only justification of the attempt was the conclusion that there are conferences and conferences and that to approve or condemn the whole lot, without discrimination, was a silly and unreasonable display of peeve and temper.

Some of you experts in the conferring modes of coöperation may, for example, be strong for the meeting that promises to pool several related sciences for the advancement of a common problem (say the vestibular functions or hearing) and at the same time you may turn thumbs down on the sort that takes such a big topic as 'regional phenomena' or 'human conservation' in its teeth to shake out an ultimate significance, or on the sort that tries to put an amorphous and ill-defined subject into such an appealing form that fat foundations will compete for sponsorship and subsidy.

Other experts may be keen to rub together two flinty terms—such as 'personality' and 'culture' or 'x-radiation' and 'life'—in order to strike fire in the tinder of well-dessicated skull-pans. Some experts on conferring will scorn the kind of assembled congregate which merely collects, sorts, and bibliographs or which issues rules for editors and publishers, or the sort that undertakes to define all the meanings of the phrase 'individual difference' or 'mental development.'

Were we to huddle this minute into a conference-as-a-whole, we could doubtless secure a vote on the thrilling questions: 'What do you believe about the rat's cortical organization?' and 'What do you *now* think about the immediate analysis of all ethnologists by the doctors of the soul?'—a poll that would certainly excite the envy of all professional poll-catchers and literary digesters.

But can we not deal more effectively with this perennial problem of consolidated researching than simply to express our disagreement on the varieties of conference, the critical, organizing, assorting, discovering, money-seeking, truth-finding, and so-on varieties?

May I take a few instances which have recently come under my observation in order to extract from them—if I can—some modest deductions upon feasible means of conjoining our efforts in research? (Since you have all been invaded by some strain or other of the virus which produces *the professor's conjunctivitis*, it would be just as appropriate for any one of you so to instance and so to deduce. Possibly my older infections and my higher immunity may speak for me in proposing to occupy your time still further.)

To speak without constraint upon wild and domesticated conferences in which one has participated would be painfully boring. I shall instead refer but briefly to a small number of attempts to advance scientific problems in the life sciences by one form or another of a socialized method.

The first—perhaps the most typical—of the conferring types of conjunction may be illustrated by a meeting recently held for a week at Cornell. Twelve young biologists from Yale, Columbia, Cornell, California, Western Reserve, Chicago institutions, Washington University, and the Rockefeller Institute for Medical Research met to present and discuss proposals for research upon a group of problems involving in an interactive way the neural, muscular, and glandular systems and their functions.[2] The formal subjects represented included anatomy, anthropol-

[2] The men were Barth, Bucy, Darrow, Freeman, Kreezer, Krogman, Marquis, O'Leary, Page, Saul, Shock, and Zwemer.

ogy, psychology, physiology, neurology, endocrinology, and zoology. The men, who were not too young to teach or too old to wish to learn, were already distinguished in research but not yet case-hardened by long service in academic departments. The main recorded object of the meeting was "mutual appreciation and understanding" of a small group of many-sided problems. The only requirements laid upon the members were (1) that each should bring a seasoned program of research of immediate interest to the group, and (2) that each should mainly devote himself to understanding and appreciating his fellows' terms, methods, and points of view. The figure of a plane 'field of science' cut up into separate squares was discouraged and the conception of a solid mass with regional differences of density, constitution, and inertia substituted. Common housing and dining, with two limited daily sessions, bodily exercise, and spontaneously formed minor groups were important incidents. The group's unframed motto read "Tolerance, objectivity, and a desire to learn."

No audience was present, no chairman presided, and no drones buzzed in the hive. Each presentation was followed by informal discussion. Representative subjects were "Research on the Adrenal Glands," "Chemical Basis of the Primitive Nervous System," "The Mechanism of Spontaneous Activity," "Cytoarchitectonics of the Pallium," "Functions of the Cerebral Cortex," and "Electrophysiology of the Brain."

At the close of the week the men considered that the profit derived was due chiefly to their common interests, complementary trainings, consideration of seasoned problems, freedom from constraint, informal chat throughout the day and night, and the opportunity for each to learn from his fellows. Discussion was recorded, the entire proceedings typed and sent to each member, who corrected and returned for a final mimeographed draft which was completed before the end of the season and sent to the supporting Foundation.

Our next sample of conjunction is of quite a different kind. It bears upon the pressing problem of mental disorder; more specifically upon the fact that a century of our medical conceptions of disorder—as a disease of the mind, of the body, or of both—has left the problem in an extremely unsatisfactory state. An inquiry into the nature, history, cause, and cure of the disorders first took the form of judicial review of present state and knowledge by bringing facts and theories before a competent board of judges for opinion and recommendation. When the method proved infeasible on account of expense and the difficulties of securing competent and unbiased judges, the more modest device was tried of asking representative psychiatrists to present the main points of view in the subject and representatives of the sciences of life to set forth methods and results of research in their several fields, results which appeared to be applicable to the understanding and conquest of the disorders. Five psychiatrists expounded and nearly a dozen-and-a-half spoke for the sciences of life, while several others discussed social, therapeutic, and cultural topics.

This method involved a sort of arranged conjunction. Instead of conferences of the group, which proved not to be practicable, a hundred or more one-to-one conferences between the general planner of the study and the individual writers were informally held over two years' time. Moreover, the psychiatrists read each other as they wrote and all those who sought to bring biological knowledge to the solution of the problem also wrote in the light of what the psychiatrists had said and according to a general plan laid down in the investigation.

In view of the welter of contending doctrines, cults, and theories in mental medicine, it had seemed to the present speaker and to Dr. Cowdry, who represented the medical sciences, that the case would be forlorn indeed if it appeared that the basal sciences of life—anatomy, physiology, cytology, embryology, neurology, psychology, and the rest—were unable to describe the elementary facts of the disordered and deranged organism and the primary causes of disorder, and—wanting those facts—were unable so much as to prescribe approved methods of research for producing this information in due course of time.

The results of this peculiar form of coöperation are not revolutionary,[3] though two-score competent men could scarcely labor concordantly for two years on a real and back-breaking problem without illuminating it. On the positive side, it is not too much to say that much new material and several methods of research in the life-sciences have been turned to new account in this direction. And it is just as important to have discovered that medical art and practice, current conceptions of the neural functions, the best of our general and special physiologies, and all of our psychologies—not to mention speculative systems and theories—are still essentially inadequate to the problem of the disorders, either the more serious disorders of our mental hospitals or the more numerous cases of insane living in the open community.

The clearing away of tradition and of the sterile rivalry of schools is plainly called for in order that a more empirical and factual account of human living and of its development and decline may be written. Especially should the concept of 'disease' in this context—the most influential here for a century—be sharply scrutinized. One possible reason why medicine has failed—failed in description, failed in etiology, failed in cure, and failed in prevention—may rest just in this concept of disease, which has been notably useful in other fields of medical practice. I very gravely doubt whether we shall solve our problem so long as disease and pathology are used as primary and not as accessory concepts in the psychiatrical field. A new reapportionment, without prejudices, is called for. The biological, the biographical, the social, and the cultural life of man and of related animal forms must be viewed concordantly and with due attention to all the factors involved in sane and in defective living. A new kind and degree of integration, a cleaner, more understanding, and more enlightened conjunction among the natural sciences and the cultural studies are emphatically called for. Here, as in several other vital and persistent problems touching man's life, institutions intrude themselves between scientifically attested fact and application to life. Like the corresponding organization in finance, government, the religious life, law, and the fine arts, the highly organized institution of medicine—which would prefer to be regarded as a science—uses its social and political powers to sustain and to defend its arts of practice. Like the corresponding institution of science, vocal and legislative in its A.A.A.S.—an entity quite unlike the working and researching sciences themselves—the medical institution has always to be reckoned with in any objective proposal to modify, and especially to limit, its domain and its activities. This is the common case of all human institutions of this order; they have made investments; they own property; and they are therefore highly influential in all matters of revision. A reassessment

[3] See the volume, *The Problem of Mental Disorder*, 1934.

of the interests of medicine in the disorders will have to be made, then, outside the medical institutes and upon more neutral ground.

Receding from a more desperate problem to one of much smaller dimensions, one which nevertheless calls for some form of wise interplay among the sciences, we come upon the nervous system and its psychological implications. Here again matters are fairly unsatisfactory. The old search for mind among the neurones has ended in a sorry way. To be sure, a medico still reports now and then the seat of the soul, the hearth where consciousness brightly burns, a sleep center, or a subterranean hiding-place of the emotions. But those flaming announcements do nót stir us as they once did. Their magic has lost its thrill.

On the other side, I am not sure that the more modern compact by psychologist and neurologist 'to pool everything' and go in for 'straight mechanism' really solves the difficulty of neuro-psychological conjunction. We know vastly more about excitation, conduction, latency, rate, functional integration, and so on, than the old neurology knew; but how your bodies at this moment contrive to see this room, note the slow passage of time, understand the topic, longingly anticipate the final sentence, neurology will still hardly undertake to expound. So slight an organic event as that which sets you now fuming at my unorthodox views on neurology, your neurologist can tell you only in a caricaturing way which mumbles something about autonomic and hypothalamus. And the reason why we know next to nothing about these plain facts of living is that many good psychologists know next to nothing about them themselves, and instead of putting sound psychological questions to the neurologist, the psychologist turns functional and operative neurologist himself, denying any specific merit or significance to the name 'psychologist,' which he is likely to retain only because he once drew fellowship funds or researched under a doctoral committee so entitled. Some concession may be made to the language of tones, noises, lights, colors, and the like; but these are the merest shreds and tatters of functional performance without which we should *have* no neurology, no science, and indeed no language, no conjunctive research, and also alas! no Institute of Human Relations.

When once again neurologist and psychologist mutually support each other with related facts upon the performances of the body, we may expect to have—quite without the magic of consciousness, dynamic agent, or the unconscious—a much fuller knowledge than we now possess of a large class of these organic operations. A fellow neurologist and I are now playing at this conjunctive game of writing in parallel, he the neural functions of the central nervous system and I the psychological functions of perceiving, remembering, emoving, comprehending, and the rest. My neurologist has had to re-write four times before he eliminated all of the shop-worn non-neural terms—'sensible,' 'sensation,' 'color,' 'pain,' 'voluntary,' 'space-perception,' 'discrimination,' 'feeling,' 'emotion,' and the like being most tenacious in his bilingual but orthodox description. On my side, I have agreed to forswear all mysterious forces and agencies, all references to mind and consciousness, and to operate my functions only by authentic organs, tracts, specific mechanisms, and other bodily devices so far as my neurological neighbor and his kind are able to attest and to describe them. Thus we attack a common problem, each using his own concepts, training, and methods, but both profiting by a sympathetic insight into the work of this limited fellowship.

Since the current mode of attack upon the problem of growth and development in Indian children, which has engaged me in a minor way for several years, is of the con-

junctive sort, I must ask you to let me say a word about it. It was brought to N.R.C., a foundling in need of parental support, by a member of the Yale Medical Faculty, Dr. Aberle. In five years of research among Pueblo, Zuni, and Hopi, it has collected increments of stature, weight, and the bodily dimensions for many hundred Indian children. It has repeated year by year the x-raying of joints, the general physical examination, tests of tuberculosis with chest films, an account of diet, with extra-vitamin feeding over many months, studies of teeth, palmar markings and hair, infant and maternal mortality, and the solution of psychological problems presented in parallel to Indian, Spanish-American, and White at like ages and with schooling controlled. On the psychological side, no attempt was made to compete with the white man's 'intelligence,' as inferred from clerical tests taken from an alien culture. Instead, occasions were set before all children alike, occasions which aroused a problem in the child and impelled him spontaneously to seek a solution. A beginning has been made at comparing the great frameworks within which cultures differentially operate—space, time, quality, the constructs of nature and of force, and the specific characters of the human life-course in its social setting. The study calls for a closer, more intelligent, and more sympathetic conjunction of psychologist and ethnologist than has yet been secured.

When we touch ethnological problems, an entirely new demand for coöperative method appears, the integration of the various social studies in the interest of common and related problems of investigation. Of conjunction in this great field, within which a part of your own work is done, I know very little. Approaching it from one side, a seminary of mine is trying to envisage enough of anthropology to be intelligent upon those places where psychology touches this neighboring field. The introduction to our meetings of sociologist and ethnologist has greatly helped; but it appears that much of that preliminary survey which is a cleaning up of the ground and the setting of marks which can be read by psychologist and anthropologist alike still remains to be done. Linguistic difficulties also remain. All parties must purify their speech, discard obsolete terms, cast useless theories in the graveyard, and generously attempt to learn each the other's language and respect each the other's problems.

Since every people lives in and by its own psychological products, products which are in part specific to itself, it would seem to be the responsibility of the psychologist—if he is ever able to read off the product and relate it to the functional operations of the organism, as imbedded in his culture—to give an account of the active organism as it moves among its fellows and of the residues of cultural history. To be a real aid to the ethnologist he will certainly have to go beneath the superficialities of 'social response' and beneath the trite categories of 'inherited predisposition,' 'instincts,' 'attitudes,' and 'libidinous urges.'

Last week I observed a great conjunctive display just now begun in the social sciences. It is a socio-economic investigation within the Soil Conservation Service, charged with planning and developing a project for the Office of Indian Affairs. The study, which some of you doubtless know better than I, is based upon the Federal Government's attempt to reclaim and to utilize for the Indians of the Southwest vast tracts of waste, eroded, and overgrazed lands stretching from the Rio Grande Valley to the Californian border and from Utah to Mexico. As a complement to physical restoration and use stands the human problem of rehabilitation. A socio-economic group of about thirty trained men and women has just gone into the Pima, Papago, and Walapai reservations in Arizona.

Here the initial task of group-study is planning. After weeks of group-incubation we all went the other day to a frosty desert in Northeastern Arizona to spend the night at a Navajo Fire Dance while one member remained in the empty offices to realize from the incubative brooding. The second phase, just begun by this group of sociologists and economists, will imply a more scattered but nevertheless still conjunctive study of the Indian populations in the three areas mentioned, always keeping in view the integral relation of a human culture to the use and usifruct of the land. The hope of the leaders is that life may be enlarged and enriched without essential damage done to the native 'life-values of the people.' Whatever the outcome of the undertaking, the problem presents to us a fairly distinctive kind of coöperative study involving several of the social sciences, a kind with which many of you are familiar. In our present context, the study is notable for its earlier *planning* phase of conjunction with a later *executive* phase of actual contact with people and culture, to be followed still later by a *summarizing and recapitulating* phase of drawing together results and extracting conclusions. The conjunctive study is also distinctive in its aim to effect a new and more fruitful integration of men with their earthly resources and attachments.

This study follows a slower and more methodical examination of the Northern New Mexican Watershed of the Rio Grande, a long-term study now in course for some time, chiefly devised by one man and based upon a detailed examination of the land, its topography, coverage, precipitation, fertility, erosion, conservation, human occupation by three diverse cultures, and modes of its improvement for man's needs. Here conjunction, which is of the coördinating kind, follows the plan and program of one man, fortunately a man well-trained in the methods of research and thoroughly familiar with land and people.

To have these two varieties of conjunctive study proceeding in parallel upon the same general problem should—in the course of years—prove to be of great importance to our own subject.

I have already gone too far, for I have gone beyond my depth; I have trespassed beyond the limits of my knowledge. May I nevertheless take another five-minutes to bring together for the sake of our topic whatever loose ends and random instances I have collected?

At least three unlike forms of conjunctive research may be distinguished. The first may be called the *Primed Non-Polarized Form*. It is illustrated by the Cornell Conference of Biologists. All members were individually researching upon related and interlocked problems and met for mutual understanding and for a recasting of their studies.

The second is the *Bi-polar Form*, as sustained, *e.g.* between neurologist and psychologist. It implies a difference between the two members with respect to training, concepts and methods, but also a complementary give-and-take which mutually supports and gives a new significance alike to neurology and to psychology. In its most fruitful form I should say that it entirely ignores the ancient 'mind-body problem' and proceeds on a factual and empirical level of research. The coöperation of ethnologist and psychologist is of the same type of bi-polarity save that here the most natural form has an entire group at each pole, integrated ethnologists in the one group and integrated psychologists in the other. The main object of conjunction (as before) is the enrichment of both fields by common regard and sympathetic understanding of their intimate relations.

The third form of scientific conjunction may be called *Centrifugal*. Here the primary activity is outward, spokewise, from a center (usually an individual with a well-integrated topic and program). Here fall such investigations as that recounted for the mental disorders—where inherent difficulty, a multitude of authorities and interests, and former failure, call for clarification, objective statement of resources, and a re-distribution of responsibilities and functions. The growth of Indian children is of the same general form, and so is the great one-man plan for rehabilitating the Indian peoples on their reclaimed lands.

To attempt to set relative values on these forms of conjunctive and socialized research would be foolish. One form is suitable for one problem and another for another. There is a temptation to apply a principle of social psychology and so to extol the coöperative functions of man. If we did, we should magnify the unpolarized form of primed conference as most highly socialized. But against this temptation we must interject two considerations; the first that pooling never creates new knowledge and seldom new wisdom, and the second that hard or elaborative thinking—assumed to be the backbone and sinew of the sciences—is of all the psychological functions least socialized. Every thought-problem is one's own unshared problem and every thought-solution is wrought out by a single living organism. The circumstance which redeems the unpolarized conference is therefore that individual thinking, together with the gift of exposition, lies behind the conferring, which grants an opportunity to reform, illumine, and broaden the individual problems laid before the group.

In short, nothing takes the place—whether in the sciences or out of them—of competent individual thinking on real problems sustained by well-trained human adults. That understood we may nevertheless say without undue optimism that social conjunction at certain stages of research with certain problems and with trained men and women may notably advance our group of the sciences.

There is, as I think, one caution which may appropriately be entered in closing. In breaking down boundaries within the sciences of life and in denying the sacredness of departments as advertised in university announcements there is a danger of losing our master-mechanics and also of slurring real and important distinctions. Pooling can go too far. A man *may be* a good physiologist, neurologist, psychologist, anatomist, endocrinologist, and cerebral surgeon. I have never seen one, one who knew in fair perspective the history and accomplishments of these several subjects and of the disciplined and useful relations among them. As we stand in a temporary reaction against specialization, there is danger of making a faddish virtue of the dead-level. Respect for differences in skill, knowledge, method, and even linguistic terms, still has its value, and the best combination of highly individual with socialized and coöperative work in the sciences is still, as I believe, to be based upon this respect and upon the sympathetic insight which proceeds from it.

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TEMPORAL PERCEPTION AND OPERATIONISM

It is the purpose of this note to show how the classical problem of temporal perception turns out to be specious when it is considered with respect to the operational procedures by which perception is known, and thus how it forms a striking instance

of the failure of immediate experience successfully to play the rôle of the content of consciousness.

The fundamental problem of temporal perception appears in the question: Can *duration* be an *immediate experience?* In general the answer, from the Wundtian to the Gestalt school, seems to have been Yes. An observer reports that a short duration is quite as "immediately given" as a short extension.[1] A pattern of successive metronome clicks has unity, and thus, if immediately given at all, must be immediately given as a whole.[2] If an observer can recognize, without counting, thirty-two successive clicks as distinct from thirty-one or thirty-three, then in a sense these clicks form a "unitary whole" within the "range of consciousness" and are "immediately given."[3] The conception of a conscious present is founded on similar facts. The present, though logically specious, nevertheless has "sensible duration"; a conscious event comes set in a "block" of duration.[4] A considerable monograph could be written on the history of the notion that a temporal perception is unitary, and in general this unity has been taken to imply immediacy of experience.

The more recent history of the problem is somewhat as follows. There was implicit in the experimental psychology of the nineteenth century a punctiform elementism. The stimulation of two different retinal points was thought of as giving rise to two distinct sensations, each with its given quality and intensity. This view implies that a perceived line would be a row of sensations. Although Külpe and Titchener added extension and duration to the list of sensory attributes, it is doubtful whether this punctiform analysis, so consistent with physiological analysis into receptors and their insulated fibers, was ever fully surrendered until Gestalt psychology attacked it. Then Külpe's point became evident: an extension is as simple, as unitary and as immediate in perception as is a quality or an intensity. It is natural to try to endow duration with all the virtues of extension, for ever since Kant it has been hard for anyone to believe that space can have any properties, except multi-dimensionality, that are not possessed by time. Moreover, we have all the evidence cited in the preceding paragraph that duration is introspectively as unitary and immediate as extension. Temporal patterns are a perceptual fact; are they not to be treated like spatial patterns? Most psychologists have treated them thus, without noting the paradox that inheres in the notion of the immediacy of a duration.

When the physiology of temporal perception is considered, psychological thinking is apt to become a little more realistic. It may be possible to think of experiential events which succeed one another throughout a second of physical time as coalesced into a unitary conscious present which is immediately given, but in physiology *immediacy* means time, and the end of a process is more immediate than

[1] Cf. J. N. Curtis, Duration and the temporal judgment, this JOURNAL, 27, 1916, 1-46.

[2] Cf. W. Wundt, *Einführung in die Psychologie,* 1911, Eng. trans., 1912, for the persistent assertion that temporal patterns are unitary wholes. Nowadays, when Gestalt psychology has set Wundt off as an elementist, we are apt to forget how much he asserted the unity of wholes.

[3] This is Wundt's view developed in relation to Dietze's experiments. Cf. Wundt, *Grundzüge der physiologischen Psychologie,* 6 ed., III, 1911, 296-345, esp. 330-337, where the unity of conscious wholes is considered for temporal perception.

[4] The phrases are James'. See W. James, *The Principles of Psychology,* I, 1890, 608-610.

its beginning with respect to whatever follows. Thus the present writer has emphasized the fact that a temporal perception, which acts as a whole, must somehow or other cumulate itself so that it has an adequate surrogate of itself when it ends. It can not be effective as a whole until it is completed, and then there must be some trace of the beginning still extant if all of the perception is to act.[5] Similarly Koffka has pointed out that a temporal perception requires cumulation in a neural surrogate at its end, and suggests that the temporal differentiation in the perception becomes spatial in the neural surrogate, presumably because spatial differentiation is about the only kind of differentiation that the brain is known to be able to provide.[6]

These latter views reflect the operational envisagement of the matter. Operationism directs attention away from the fixed entities to the processes of knowing, and in the case of consciousness these processes are, in the first phases of their occurrence, the physiological processes of introspective report. In other words the problem as to whether a perceived duration must be cumulated in order to be perceived as a whole evaporates from the operational point of view. Instead we note that there is no meaning to the assertion of the existence of a temporal perception, except that certain physiological events exist prior to the report and that their differentiation is such as the report can adequately indicate. The paradox is gone because there is no longer any requirement of immediacy. For immediacy we substitute physiological *continuity,* which is probably all there is to this ancient psychological concept in any context.

This conclusion follows so simply that it may be hard for the reader to see that anything important has been said. I make bold, therefore, to suggest specifically that operationism leads to the following three conclusions, which are positive and univocal and which may be substituted for the equivocal paradox of the immediately experienced duration.

(1) A perceived duration or temporal pattern is a psychological entity that is inferred from and defined by certain operations of introspective report, which adequately imply the differentiation of the perception. This "behavioral" inversion of the point of view toward experience makes the perception, not a private immediate experience, but a psychological *construct* which (in a rat or a person or myself) is just as public as is any other convincing inference from data.

(2) The term *immediate* as applied to temporal experience turns out to be meaningless. If *immediacy* means *without lapse of time,* then the word can not be applied to the perception of an enduring event. If it means *without intermediate processes between the event and the knowledge of it,* then it can not apply to any event. In any case the beginning and the end of a unitary temporal whole would have to be differently immediate. Instead of *immediate* we may say *continuous,* meaning that the process between the event and its manifestation in report is uninterrupted. This is obviously the proper meaning of the old term *immediate.* On the other hand, observational processes that are interrupted, as when a second knowing organism observes a first, or as when data are kept to be combined later with other data to yield a general function—such discontinuous processes are plainly what were said to be *mediate* in the old-fashioned sense.

(3) What is thus said of time may be said of everything else. If we could

[5] E. G. Boring, *The Physical Dimensions of Consciousness,* 1933, 129, 136-146.
[6] K. Koffka, *Principles of Gestalt Psychology,* 1935, 438-464, esp. 452.

argue that the immediate perception of extension points toward the immediate perception of duration, then we can argue that the conclusion that duration can not be immediately perceived points toward a conclusion that extension can not be immediately perceived. Besides, the perceptions of extension and intensity and quality are all just as much processes as the perception of duration, and just as much involved in mediacy. Like duration they may be thought of as constructs based upon the operations which yield them.

Thus we get altogether rid of the troublesome notion of the immediacy of experience. If it applied to extension it had to apply to duration, and, applied to duration, it was both an impossible paradox and an obvious introspective fact. The trouble seems to be that what is introspectively obvious is not necessarily true.

Harvard University EDWIN G. BORING

THE PALMAR GALVANIC SKIN REFLEX (SWEATING) AND PARASYMPATHETIC ACTIVITY

That the palmar sweat glands are innervated from the sympathetic chain has been demonstrated beyond doubt by many investigators. Nevertheless, the common identification of palmar sweating and galvanic activity with the functions of the thoracicolumbar sympathetic nervous system is open to serious question. It appears that anatomical facts have obscured several important functional considerations.

(1) The sweat glands behave pharmacologically like parasympathetic mechanisms; they are stimulated by pilocarpine, facilitated by physostigmine, and inhibited by atropine and adrenalin. (2) The neural excitation of the sweat glands, as appears from the work of Dale and Feldberg,[1] is by the liberation of an acetylcholine-like substance at the nerve ends similar to that which is demonstrated in the case of many unquestioned parasympathetic mechanisms. (3) Sweating, as shown by Hasama,[2] is most readily elicited by stimulation of the anterior hypothalamus or adjacent prechiasmic region from which Beattie,[3] Ranson, Kabat and Magoun[4] and others have elicited parasympathetic manifestations. (4) Stimulation of the region of the dorsal vagus nucleus anterior to the calamus scriptorious in the medulla also produces sweating as shown by Winkler,[5] Hasama,[6] and Foa and Peserico.[7] (5)

[1] H. H. Dale and W. Feldberg, The chemical transmission of nervous stimuli to the sweat glands of the cat, *J. Physiol.*, 81, 1934, 400-410.

[2] B. Hasama, Pharmakologische und physiologische Studien über die Schweisszentren: II. Über die Einfluss der direkten, mekanischen, thermischen, und elektrischen Reizung auf die Schweiss sowie Wärmzentren, *Arch. f. exper. Path. u. Pharmakol.*, 146, 1924, 129-161.

[3] J. Beattie, The relation of the tuber cinereum to gastric and cardiac functions, *Canad. Med. Ass. J.*, 26, 1932, 400-405.

[4] S. W. Ransom, B. S. Kabat, and H. W. Magoun, Autonomic responses to electrical stimulation of hypothalamus, preoptic region and septum, *Arch. Neurol. & Psychiat.*, 33, 1935, 467-477.

[5] F. Winkler, Die Zerebrale Beeinflussung der Schweisszekretion, *Arch. f. d. ges. Physiol.*, 125, 1908, 584-594.

[6] B. Hasama, Pharmakologische und physiologische Studien über die Schweisszentren: III. Zur Frage nach der Beeinflussung der Schweisszekretion und der Körpertemperature durch die Medulla oblongata, *Arch. f. exper. Path. u. Pharmakol.*, 153, 1930, 257-290.

[7] C. Foa and E. Peserico, Le vie del reflesse neurogalvanico, *Arch. de Fisiol*, 21, 1932, 119-130.

Pituitrin injected (or secreted?) into the cerebral ventricles as shown by Cushing[8] and others, produces sweating along with flushing, lachrymation, salivation, temperature drop, and gastrointestinal symptoms typical of parasympathetic activity. (6) Stimulation of the neighborhood of the motor area of the cerebral cortex (premotor area?) which Bechterew,[9] Winkler,[10] Langworthy and Richter,[11] and Wang and Lu[12] found to produce sweating of the footpads of the cat also gives rise, as shown by Fulton, Kennard, and Watts[13] and others, to vasodilation, and gastrointestinal activity and other predominantly parasympathetic manifestations. (7) The extirpation of the premotor area also causes an initial period of profuse sweating in association with typical parasympathetic effects.[14] (8) Our studies of the influence of various types of stimuli on human subjects have shown "disturbing ideational" stimuli occasioning marked rise in blood pressure (a sympathico-adrenal effect?) to be typically attended by relatively small galvanic responses whereas familiar startling "sensory stimuli" occasioning little rise or a fall in blood pressure and slowing of the heart (a parasympathetic effect?) are typically attended by large galvanic responses.[15]

Since it has been shown that there is no necessary relation between the galvanic skin reflex and vasoconstriction,[16] and since Wang and Lu[17] have shown that there is no demonstrable inhibitory effect on galvanic activity from stimulation of the dorsal spinal roots,[18] the phenomenon appears to be symptomatic of parasympathetic or "cholinergic" processes rather than of sympathetic or "adrenergic" activity. In other words, it appears that we have in this mechanism an instance of what is by no means the unique phenomenon of functionally parasympathetic or "cholinergic" fibers arising in the thoracico-lumbar sympathetic system.[19] It is of note that the

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[8] H. Cushing, The reaction to posterior pituitary extract (pituitrin) when introduced into the cerebral ventricles, *Proc. Nat. Acad. Sci.*, 17, 1931, 163-180.

[9] W. Bechterew, Der Einfluss der Hirnrinde auf die Thränen, Schweiss und Harnabsonderung, *Arch. f. Anat. u. Physiol., Physiol. Abth.*, 1905, 297-305.

[10] Winkler, *op. cit.*

[11] O. C. Langworthy and C. P. Richter, The influence of efferent cerebral pathways upon the sympathetic nervous system, *Brain*, 53, 1930, 178-198.

[12] G. H. Wang and T. W. Lu, Galvanic skin reflex induced in the cat by stimulation of the motor area of the cerebral cortex, *Chinese J. Physiol.*, 4, 1930, 303-326.

[13] J. F. Fulton, Some functions of the cerebral cortex: I. Autonomic representation of the cerebral cortex, *J. Mich. State Med. Soc.*, 33, 1934, 175 ff. J. F. Fulton, M. A. Kennard and J. W. Watts, Autonomic representation in the cerebral cortex, *Amer. J. Physiol.*, 109, 1934, 37.

[14] M. A. Kennard, H. R. Viets and J. F. Fulton, The syndrome of the premotor cortex in man: Impairment of skilled movements, forced grasping, spasticity, and vasomotor disturbance, *Brain*, 57, 1934, 69-83.

[15] C. W. Darrow, Electrical and circulatory responses to brief sensory and ideational stimuli, *J. Exper. Psychol.*, 12, 1934, 267-299; C. W. Darrow and A. P. Solomon, Galvanic skin reflex and blood pressure in psychotic states, *Arch. Neurol. & Psychiat.*, 32, 1934, 273-299.

[16] Darrow, The galvanic skin reflex and finger volume changes, *Amer. J. Physiol.*, 88, 1929, 219-299.

[17] F. H. Wang and T. W. Lu, On "inhibition" of the secretion of sweat in the cat by stimulation of dorsal nerve-roots, *Chinese J. Physiol.*, 4, 1930, 175-182.

[18] Effect of dorsal spinal root stimulation on the amount of visible perspiration is apparently due primarily to rapid evaporation occasioned by vasodilatation and increased temperature.

[19] A. Rosenbleuth and W. B. Cannon, The chemical mediation of sympathetic vasodilator nerve impulses, *Amer. J. Physiol.*, 112, 1935, 33-40.

fibers in this instance are apparently postganglionic in character.

The importance of these considerations lies in the fact that many clinical, physiological, and psychological galvanic skin reflex phenomena, as, for example, the typically high palmar conductance in anxiety neuroses where parasympathetic symptoms are frequent,[20] attain new significance when recognized as functionally parasympathetic manifestations.

Institute for Juvenile Research CHESTER W. DARROW
Chicago, Illinois

A NOTE ON THE SCORING OF MOVEMENT IN THE RORSCHACH TEST

Following Rorschach's example, those who use his test seem generally to have considered that true movement (M) responses occur only when the content of the responses is human or human-like. Vernon, for example, writes: "B (movement) responses almost invariably refer only to human figures."[1]

In an experiment with two groups of college students,[2] (100 *Ss* in all), the present writer found that M responses with non-human content (henceforth designated M_x) occurred about as frequently as M responses with human content (henceforth designated M_h).[3] While this study failed to yield any satisfactory criterion by which one might validate either M_h or M_x scores, several items of evidence did appear which suggest that in calculating the M score it might be better to include both M_h and M_x responses.

(1) Reports were obtained from the *Ss* concerning their experiences in perceiving the M_h and M_x items. These reports failed to indicate any consistent differentiation between the processes involved in the M_h and M_x responses. Many reports indicated that M_x involved a primary sensing of motion and empathic feeling.

(2) A statistical analysis of the results also indicates that M_h and M_x are related. In one of the groups, the correlation between M_h and M_x was $0.585 \pm .069$; in the other group, the correlation was $0.546 \pm .062$. Since it is apparent that those factors which determined the content (human or non-human) of these responses would not be correlated, it would seem reasonable to suppose that the relation which exists between M_h and M_x is due chiefly to the movement quality involved in the responses. In other words, it seems probable that M_x and M_h are both determined partially by the movement factor—which we attempt to measure by the Rorschach Test—and partially by other factors, which for present considerations are extraneous. If this line of reasoning is correct, one might well expect that the total movement score ($M_t = M_h + M_x$) would yield a better measure of the movement factor than could be obtained by either M_h or M_x alone.[4]

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[20] A. P. Solomon and T. L. Fentress, Galvanic skin reflex and blood pressure reactions in the psychoneuroses, *J. Nerv. & Ment. Dis.*, 80, 1934, 163-182.

[1] P. E. Vernon, The Rorschach ink-blot test, *Brit. J. Med. Psychol.*, 1933, 13, 104-105.

[2] A fuller report of the procedure and results of this experiment is now in press.

[3] In Group I there was an average M_h of 2.7 and an average M_x of 1.9. In Group II there was an average M_h of 2.3 and an average M_x of 3.3.

[4] The correlations between M_h and M_t were: Group I, $0.938 \pm .013$; Group II, $0.863 \pm .023$. These correlations, while containing a spurious element, show to what extent the M_t scores agree with the M_h scores.

(3) That the M_t score is a better measure is indicated further by the fact that the M_t scores are found to be consistently more reliable than the M_h scores. Split-half coefficients (with estimation by Spearman-Brown formula for full length tests) were: Group I: M_h, 0.853; M_t, 0.919; Group II: M_h, 0.603; M_t, 0.768.

Reliability was also measured by calculating M/C scores (C = total color score) for split-halves and translating these by a definite formula into one of the following diagnoses: extratensive, introversive, coartative, ambiequal, dilated. When M_t/C scores were used, the diagnoses for split-halves corresponded exactly in 55% of the cases of each group. When M_h/C scores were used, the correspondence was 55% in Group I and 45% in Group II.

Such evidence as we find, therefore, points to the conclusion that a better measure of M is obtained by including all responses which otherwise correspond to Rorschach's concept of movement response, whether their content is human or non-human. A final test of this conclusion, of course, must await the finding of a satisfactory validating criterion.[5]

University of Nebraska GEORGE R. THORNTON

THE ELEVENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its eleventh annual meeting at Northwestern University, Evanston, Illinois, on Friday and Saturday, April 24 and 25, 1936. The attendance was the largest in the Association's history, with 281 members and 95 non-members registering. The program consisted of 13 regular sessions devoted to personality, child development, electrophysiology, learning, psychopathology, nervous functions, applied, comparative, esthetics, sensory and perceptual processes, and psychometrics. The most popular topics, in point of number of papers, were psychopathology and nerve function.

In addition to the regular session, 11 symposia were held Friday evening on topics ranging from Methods of Teaching Psychology to Highway Safety and Speech Disabilities. The topics were selected by a mail vote of the members. Consequently, the chairmen of the symposia found it possible to arrange discussions ahead of time. These discussions were generally felt to be among the most valuable features of the program.

On Saturday afternoon, a special address was delivered by Dr. S. W. Ranson, of the Neurological Institute of Northwestern University, on "The Hypothalamus: Its Rôle in Emotional Reactions and Sleep." The usual session for reports from laboratories was held Saturday, in which synoptic reports of work in progress were given by authorized representatives.

At the annual business meeting on Friday evening, it was announced that the next annual meeting of the society would be held at the University of Illinois, Urbana, Illinois. The recommendation of the program committee, that the time of the meeting be extended to three days, was defeated but the recommendation that in the future any non-member wishing to read a paper must be sponsored by a member, whose name must appear on the program as sponsor, was accepted. It was also decided that hereafter all notices of meetings, ballots, and calls for papers

[5] The author is indebted to Professor J. P. Guilford and to Dr. J. McV. Hunt for helpful criticisms and suggestions.

should be sent to active members only, and that there would be a charge for extra copies of the program. The Council recommended and the Association voted to revise the membership rules to read that an applicant must be a member of the American Psychological Association, or otherwise he must present an application endorsed by an active member of the Midwestern Psychological Association, with a statement of the applicant's qualifications including his publications, if any, and must be passed upon by the council. Such applications will, hereafter, be received up to March first, to be acted on by the council and voted upon by the Association at their annual meeting. The Association voted against publishing the abstracts of the papers read at the annual meetings. The election was announced of Professor H. A. ~~Carr~~ (Chicago) as president of the Association for 1937, and of Professor J. P. Guilford (Nebraska) as member of the council for the term 1936-39. A memorial to the late Professor Joseph Peterson was read by Dr. H. B. English.

At the annual banquet, on Saturday evening, at which more than 150 persons were served, the presidential address was delivered by Professor Christian A. Ruckmick (Iowa) on the subject, "Psychology Tomorrow," in which he ventured some modest predictions of the future possibilities of the science on the basis of past performance and present trends, and expressed the hope that psychology would demonstrate its worth in spite of disparaging evaluations by certain contemporary critics. Professor A. R. Gilliland (Northwestern) welcomed the Association back to Northwestern, its birthplace.

University of Chicago ARTHUR G. BILLS

THE WORCESTER MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The thirty-second annual meeting of The Society of Experimental Psychologists was held this spring at Clark University on April 9 and 10. Twenty-two members from fourteen institutions were in attendance. After a short business session, at which it was decided to incorporate the Society, informal reports of work in progress were received from the following laboratories: Brown, Chicago, Clark, Columbia, Cornell, Harvard, New York Psychiatric Institute, North Carolina, Pennsylvania, Princeton, Smith, Washington (St. Louis), Wellesley, and Yale.

On the evening of the 9th, at a dinner given the members of the Society by Clark University, President W. W. Atwood extended the University's welcome and greetings. After a response by the chairman of the Society, Professor W. S. Hunter of Clark University, the Howard Crosby Warren Medal for 1936 was awarded jointly to Professors Ernest G. Wever and Charles W. Bray of Princeton University for their contribution to the knowledge of auditory function.

This occasion marks the first award of the medal which was established, "for outstanding research in psychology," by Mrs. Catherine C. Warren in memory of her husband, Howard Crosby Warren, the late professor of psychology at Princeton University. Professor Warren was one of the founders of the Society in 1904, and the first chairman of the Society following its reorganization in 1929.

The next annual meeting of the Society will be held at Smith College, Northampton, Mass., on Thursday and Friday, April 1-2, 1937. Professor Kurt Koffka of Smith College was elected chairman of the Society for the ensuing year.

K. M. D.

BOOK REVIEWS

Principles of Gestalt Psychology. By K. KOFFKA. New York, Harcourt Brace & Co., 1935. Pp. xi, 720.

The present volume will unquestionably assume a position of first importance among books which have been devoted to the problem of psychology. It is important not only in its own right. It furnishes a definitive statement of the principles which have guided the thought and experimentation of a group of men whose views have had profound influence wherever psychology has been taken seriously. Whether these principles find general acceptance or not, they can not be treated lightly, for they penetrate into practically every concern of present-day theoretical psychology. As a matter of fact, the bulk and style of the writing will effectually preclude superficial perusal. It is hard writing, certainly not designed for the layman nor for introductory students, and requires close application from beginning to end.

The program which Koffka maps out for himself (p. 67) is a large one, to which he devotes himself with energy and aplomb. His erudition is extraordinary, and his discussions of the relation of experimental findings to theory compare favorably in scholarly breadth with anything which psychology has ever produced in its whole history. It is out of the question in a review to give even an indication of the wealth of material which Koffka has treated. The reader must satisfy himself on this point. For two reasons I refrain from giving a synopsis of the chapters, or even of the book as a whole. In spite of the fact that Koffka insists that there is no "secret of the guild" which gives him or other members of the so-called "gestalt school" a special standing (p. x), it is nevertheless true that his concept of systematization differs enough from the usual variety to make it peculiarly difficult, if not unwise, for anyone not thoroughly saturated in it to attempt a brief exposition. Secondly, the space at my disposal can perhaps be more profitably devoted to the enumeration of occasional doubts and reservations than to a citation of points of agreement. I should be glad to say all the fine things which the book deserves to have said about it, but instead I shall proceed to find fault.

The nature of the behavioral environment is defined by three characteristics: shaped boundedness, dynamic properties, and constancy (p. 72). To these particular characteristics I have no objection whatever. They are as good as, and probably better than, a great many others which might be singled out. The point to be borne in mind, however, is that they are *singled out.* Gestalt psychology have levelled severe strictures against the descriptive categories used by other psychologies on the ground that they give a distorted and false picture of the object supposedly described, whereas gestalt phenomenology claims to furnish an unprejudiced and full description of direct experience (p. 73).

Now how does one know that any given description is as naïve and full as possible? Let us suppose that the object of description is Mr. *X,* who is standing, let us say, a few feet in front of me. What shall I say about him? Well, I may start by saying that he is six feet tall. It is obvious, of course, that *X* and his height are not one and the same thing. Height is only a part of *X,* and a part, moreover, which gestalt psychology would be inclined to regard as not experiential, and therefore as an artificial abstraction. I may note, instead, then, that the shape of *X* is different,

both in contour and location, from that of the neighboring bookcase. The shaped boundedness of *X* is again not identical with *X* himself, but it is one of his aspects which for gestalt psychology is more real and permissible than his height. I may be aware that he is smiling (dynamic property) and that when he walks out into the hall his size remains practically the same (constancy). These two characteristics are acceptable, but if I mention the hue of his face or the brightness of his necktie, the descriptions are ruled out because of their sensationalistic bias.

It is not at all clear to me in what way gestalt psychology differs in its descriptive procedures from any other kind of psychology. Whatever can possibly be said *about* an object can not be identical with the object itself. The ineffable freshness and richness of the original object must of necessity vanish when it becomes object of scientific operation. The intuitions of artists may capture the first fine flush of reality, but the more prosaic task of science is to probe beneath the surface in order to find out the conditions which govern the appearance and behavior of objects. Among these abstracted conditions reality itself does not appear. To condemn these conditions because they differ from the initial object of observation is to condemn the whole structure of science—a radical step which Koffka surely does not mean to recommend. But it is a move in this direction, whether intentional or not, which every psychologist makes when he deplores the cold abstractions of descriptive systems. For not a single thing can be said about *X* which is not an abstraction. The psychology of his love-life, when it is known, will be no less unromantic than the explanations of his pupillary reflexes or his discriminations of tonal loudness.

The history of psychology has been filled with the lamentations of those who have cried out that psychology must turn away from its neat conceits and cunning constructions, back to the warmth and glow of human nature and personality. I am willing to prophesy that the future of psychology will show that when human personality finally yields to scientific treatment, it will be couched in a system of complicated abstractions of which the poet, who perhaps intuits personality best, will have only the fuzziest comprehension. "I remember," says Koffka, "an episode in my student days. A colleague of mine with whom I was going home asked me the question: 'Have you any idea where the psychology we are learning is leading us?' I had no answer to that question, and my colleague, after taking his doctor's degree, gave up psychology as a profession and is today a well-known author. But I was less honest and less capable, and so I stuck to my job" (p. 53). Undoubtedly few psychologists have been completely free from wistful moments of regret that so many laymen, as well as novelists, seem to have such a lively "understanding" of human nature. Many laymen are also possessed of a better knowledge, from a naïve phenomenological point of view, of the heavens at night than professional astronomers can boast of. Many people know the habits of alley cats better than do biologists. From which it does not follow that dishonesty and incapability have kept scientists in their place.

One of the descriptive abstractions with which Koffka is greatly concerned is that of the so-called phenomenal constancies. His treatment of this very complicated topic is brilliant, and is especially timely in view of its strange neglect by American psychology. It is, therefore, all the more disturbing to have to confess to a feeling of keen disappointment at the manner in which Koffka seems to let his readers down after their hopes have been raised. Illustrative material from the size-constancies is drawn upon again and again in demonstrating the inadequacy of earlier theories to

the facts of the behavioral environment, and the reader is then invited to prepare himself for the true and final answer (pp. 98, 105).

What is the true solution of size-constancy? To this question I was able to find no answer. A brief statement regarding the invariants of constancy of size is far from lucid, and it is made more complicated by the introduction of the projected after-image for purposes of illustration (p. 222). The apparent doubling in the size of the image is related to the invariants between it and the drawing of the tunnel, but what determines the apparent equality of the two lines of the tunnel? "For size as for shape *some* effect will be invariant for a given stimulus, and this effect will be some combination of size and distance." But "the form of this combination has to be worked out in future experiments" (p. 236). So we learn that the true solution must wait upon future experimentation. Such a discovery is not a novel one in psychology, but it is disconcerting to come abruptly against it in a book which holds out hopes of new and final solutions for problems to which an older type of theorizing was said to be hopelessly inadequate.

These comments are not intended to minimize the importance of Koffka's analysis of the *facts* of size-constancy. They refer rather to the general weakness of present-day theories, gestalt theory not excluded, in the face of these particular facts. Jaensch, whom Koffka does not cite in this connection, has perhaps made as good a guess as anyone in suggesting a generalization combining Emmert's law and the Hillebrand functions. Retinal images decrease, after-images increase in size with distance. These two mechanisms working against each other might serve to maintain the size of receding objects in the behavioral environment at an intermediate value (constancy). The suggestion at least has the advantage of being concrete and quite possibly susceptible of experimental disproof, whereas explanations of visual organization in terms of field pressures and strains must for a long time enjoy the dubious distinction of perhaps being right without anyone's knowing it.

Koffka's reluctance to define some of his terms leads to many verbal obscurities. One example must suffice. The word *organization* occurs, I had almost said, on every page, but in many cases it is not at all clear what it means. I take it that among other things it may mean the changes in the behavioral environment correlated with different *Aufgaben* under constant geographical conditions (p. 149). In this sense the word is apparently a name for an impressive array of facts which have been accumulating in psychology ever since Külpe's famous experiments on abstraction, and, as a name, it has no more explanatory power than any other word that might have been chosen. In other contexts, however, the use of the word seems intended to convey to the reader an explanation of facts hitherto quite inexplicable. "The fact that clangs can be analyzed, their overtones heard, under a special direction of the attention, belongs to the oldest store of knowledge our science possesses, although the actual explanation of this effect was not achieved till some years after the war. For the problem of organization was unknown to the older psychology, at least in the form in which it has followed us through this whole book. And changes in the behavioral environment, whether they be due to attention, attitude, or what not, are primarily changes of organization. As such a factor of organization, attitudes have been most thoroughly investigated by Gottschaldt" (*sic*, p. 394 f.). In the second sentence just quoted Koffka appears to be calling upon some *principle* of organization to account for the analysis of clangs. But in the next sentence the different *results* of analysis *are* the organization, from which verbal ambiguity one may evidently be

allowed to draw the conclusion that the organization of the behavioral environment is best explained by organization.

In his well-known experiments on the associative equivalent of determination, Ach tried to devise a scheme for measuring the relative strengths of two opposing tendencies—not an absurd idea, even if it did originate in the baneful atmosphere of associationism. In daily life, old habits often seem to have tremendous dynamic capacity, no matter how strong the determination to get rid of them. Ach's experiments did not prove all that he hoped they would, but they did demonstrate in striking fashion that when meaningful relationships have been established between items (transposition, rhyming, superordering, part-and-whole, and the like) the course of action and thought is far more likely to be governed by these relationships than by frequency of connection between these items and other less meaningfully related ones. Ach seized upon this important fact, wrote two books on the subject some twenty-five years ago, and initiated many of the modifications which associationism has since undergone. What he was less successful in showing was that frequency of repetition could be used as a measure of the strength of a counter-determination. It is still possible, however, to interpret his tables as showing in many instances a genuine conflict between old habits and counterdetermination. How else does one explain the difference between the value 663.5 σ for inversion of syllables already learned inverted and 841 σ for inversion of syllables learned in the usual fashion? (What does Koffka mean when he says on p. 578 that Ach's results hold "only for the task of inverting and not for that of rhyming?" The value for rhyming of syllables already rhymed is 776.5 σ, while that for rhyming of syllables learned in a different way is 1132 σ.)

With a summary dismissal of Ach, Koffka "can now pass to Lewin's work, for he succeeded where Ach failed" (p. 578). Lewin repeated Ach's experiments with a number of modifications of method. "Lewin's results," says Koffka, "are in strict contradiction to those of Ach and to the assumptions which guided this author. In order to explain the contradiction Lewin introduced an entirely new theory" (p. 579). The contradiction, I think, is more apparent than real. Lewin found no reliable difference between new syllables and learned syllables with respect to the task of inverting. Exactly the same result had been turned up by Ach! The difference between new and old syllables was for Ach 14.5. But what Ach did find was a very appreciable difference between the inverting of syllables *already learned inverted* and the inverting of syllables learned in another way. This difference is important. It can not be found in Lewin's results for the simple reason that his method precluded its appearance.

Lewin contends that bonds (associations) and forces (*Aufgaben?*) can not come into conflict. Conflict can result only between forces. Inhibition of association he therefore explains in terms of a collision between an active process of identification of the syllables and the task of inverting them. The process of identification sets in slowly and becomes more disturbing as the series progresses. From which it would seem to follow that the reaction-times should be longer in the later series. His figures do not confirm this notion. His average times for all syllables in Series I on days 17, 18, 19 are 607σ, 678σ, and 569σ; for the syllables in Series II and III on the same days, 598σ, 575σ, and 560σ. The point is not important enough to labor, except insofar as it tends to make one doubt whether gestalt psychology is in possession of conclusive evidence that the theoretical position of a psychologist like Ach is no

longer tenable. To dub associations *static* and Ego-attitudes *dynamic* is only to befog the whole issue with words. It is as easy to talk dynamically about associations as it is to talk statically about the Ego, unless one defines the terms rigidly.

Koffka's dislike for associationism is due in part to the arbitrariness of the connections which it postulates. Intrinsic relations between items are more potent in the formation of connections than repetition of unrelated items. Organization thus replaces association. One of the most powerful factors in producing organization is similarity. Now association by similarity is a principle which goes back to Aristotle. Its hold upon psychological theory has been tenacious, in spite of efforts to reduce it to contiguity. Since Müller and Pilzecker, *e.g.* it has been one of the most important problems in the theory of retroactive inhibition. Yet Koffka insists that "it has gained no recognition in associationistic psychology although it was clearly pointed out and vigorously emphasized by Höffding in 1889" (p. 582). Höffding's principle assumes "that the trace *a* can be rearoused by a process, *a'*, different from, but similar to *a*, and that therefore *a'* can reproduce *b*" (p. 583). Robinson's law of assimilation assumes that "if an association between *a* and *b* has been formed, then not only *a* will be able to reproduce *b*, but also any *a'* which is similar to *a*, the strength of the reproductive tendency aroused by *a'* varying with the degree of similarity between *a'* and *a*" (p. 583). Höffding's principle, according to Koffka, is sound and constitutes a genuine explanation. Robinson's law, on the other hand, he rejects, for it "*names* what it considers to be a fact without explaining it" (p. 583). I can only invite the reader to examine the passages from which these quotations are cited in order to see whether he can find out for himself why one principle is right and the other wrong. For myself, I can discover no difference between them.

If there is any polemical spirit in the above comments, it is obvious, I hope, that it is directed not against Professor Koffka but against the ambiguities and tenebrosities of *Gestalttheorie* itself. Nor should I care to be likened to Fechner's critics who, as James said, "always feel bound, after smiting his theories, to wind up by saying that nevertheless to him belongs the imperishable glory," etc. Gestalt psychology has secured for itself an enviable position in modern thought, and no words of mine will diminish its prestige or the importance of its contributions. I could only wish, as an admirer of all of its experimental work, that its theorizing might let in a little more daylight.

Harvard University C. C. PRATT

Estudios Psicologicos: Contribucion Al Estudio Experimental de la Asociación en Niñas de 7 a 20 Años. By J. V. QUINTANA MARTINEZ. Madrid, Universidad Central. Facultad de Ciencias. Seccion de Naturales, 1935. Pp. 237.

This doctor's thesis from the University of Madrid embraces a wider range of topics than the title suggests. The purpose of the author was an experimental study of the range and variety of verbal association, as well as the time consumed in associative processes of varying complexity in girls of from 7 to 20 years of age. As an introduction, however, Dr. Quintana offers a 40 page history of the associationistic psychology which is remarkably clear and comprehensive considering its length. Although this part of the work is frankly based on Warren's *History of the Association Psychology,* it is something more than a good summary since the author presents independent historical interpretations of his own evidently based on a reading of original authorities. For example, he appears to believe that Luis Vives is

the most important figure in the early history of associationism and criticises historians of psychology for their neglect of this great Spanish writer. He is also very clear in his exposition of the views of James Mill, Bain, Spencer, and Lewes, but devotes too little space to the modern experimental studies of associative processes although the latter might seem a more logical introduction to an experimental study. (p. 229).

Dr. Quintana's experiments fall into two divisions. The first deals with the range, variety, peculiarities, and types of error in the verbal associations of 300 school girls of from 7 to 20 yr. of age. The *Ss* were divided into 12 equal age-groups of 25 each, the highest age-group, however, including girls of both 18 and 19 yr. One hundred stimulus-words, which with a couple of exceptions were the equivalents of those used in the Kent-Rosanoff tests, were employed and the *Ss* were asked to write down the first word which occurred to them and were allowed a period of 30 sec. to respond. Analysis of the results shows in general (1) a multiplicity of different associations for each stimulus-word, (2) many girls coincide in the same association, and (3) certain words evoke a maximum coincidence in response, while other words evoke a minimum coincidence.

The second series of experiments deals with the time consumed in associative processes of various degrees of difficulty. The subjects were the same as in the previous experiment but included only those from 7 to 14 yr. old. The experimental arrangement included an apparatus for the display of stimulus-words and synchronous starting of a Hipp chronoscope, and lip keys in the mouths of the *Ss* which served as interruptors stopping the chronoscope immediately upon response. The measurements are given in thousandths of seconds. It was found that the time employed in the associative process in general decreases gradually with an increase of age. More time is consumed in conditioned than in free association. Response to abstract stimulus-words requires more time than response to concrete words.

An inspection of the numerous graphs which illustrate this work suggests that most of the fluctuations in the various aspects of association considered are connected in some way with the adolescent period. The author believes that intelligence varies directly with the number and variety of associations as well as with the rapidity of the associative process, and shows how deductions from the experimental results agreed with the scholastic achievements of his subjects.

A general summary of theoretical conclusions and experimental results, in thirty-four brief propositions, facilitates study of this valuable book which is a contribution to history and theory as well as to experimental literature.

A bibliography of 96 authors but including a considerably larger number of titles is appended.

University of Cincinnati CHARLES M. DISERENS

Educational Psychology. By ALVIN E. EURICH and HERBERT A. CARROLL. Boston, D. C. Heath & Co., 1935. Pp. vii, 436.

This book has the distinction, still rare among books under the same title, of treating with intent and purpose only those aspects of psychology which are of special interest to students entering the profession of teaching. The book in no way competes with elementary psychology textbooks. The personality measurement, the differentiation of teaching, the practice of learning are the three main topics treated.

The introduction points out that any "general" improvement in scholarship is in reality only the greater efficiency following specific remedial programs which may or may not be strengthened by disciplinary actions; the latter are of secondary importance.

The main body of the text begins by saying that mathematics, in the sense of usage of statistical data, is a clarifying medium in the teacher's thought and as such a part of his psychology. The data of statistics are the result of accurate measurements. The nature of tests is therefore made clear to the reader. The term "intelligence test" is rightly frowned upon, and such particular terms as "CAVD intellect" are recommended. The authors very wisely emphasize that no school achievement test is significant unless it is related to the gains which the teacher is actually attempting to provoke. They also stress that the growth in numbers of published tests has gone far beyond the intelligent uses that have been made of them.

The testing of interest and of honesty is adequately discussed. The authors deserve praise for giving a good deal of attention to tests of esthetic abilities, even though they have to admit that some "outstanding" contributions in this field have been "largely negative" in results. The gifted child, the subnormal child and the maladjusted child are given due attention.

Learning studies, although carried on for nearly half a century, have been limited largely to the acquisition of skills or to relatively simple mental functions. The authors place before their readers the need of studies of classroom learning, which is difficult to measure. They comfort the reader by quoting from James that a knowledge of psychology does not make a good teacher.

Fixed laws of learning are not yet known. How difficult it is to define the problems and the procedures for measuring memory and forgetting, the authors make clear by many examples like that when one psychologist reports a very considerable retention during the summer vacation and another a great memory loss. For some students marks are an incentive to learning, but about the neurological mechanism in such cases nothing is known.

The teacher of educational psychology who knows his subject ought to find this textbook a valuable aid for his work. The reviewer may be riding a hobby when—as a minor matter—he calls the authors' attention to the fact that pupils may be said to have "*per*centile ranks," but that "scores" had better be called centile, decile, quartile scores, and not "*per*quartile scores," etc., unless one means to report that he "grades on a 'hundred' or a 'ten' or a 'four' basis."

University of Miami MAX F. MEYER

Child Psychiatry. By LEO KANNER. Introductions by Adolph Mayer and Edwards A. Park. Springfield, Ill., Charles C Thomas, 1935. Pp. xviii, 527.

Dr. Kanner's aim in writing *Child Psychiatry,* the first textbook on this subject in the English language, was to cover the entire field of children's personality disorders in a broad, objective, unbiased, and practical manner. In the opinion of the reviewer these objects were realized and the volume in consequence promises to become a standard book for pediatricians and child psychologists.

In the introductory chapters the author briefly evaluates several systematic approaches to child psychiatry. Psychoanalysis is condemned because of its "highly speculative, if not poetical, metapsychological structure." Adlerian organ inferiority, Jaenschian typology, the idea of focal infections, the dogma of unruly endocrines,

and even Watsonian behaviorism are considered too circumscribed, if not one-sided, to form a substructure on which to erect a psychopathology of childhood. Behavior disorders usually involve a multiplicity of interdependent factors; consequently, the total personality of any problem child must be kept in view. In short, the psychobiological approach becomes imperative. Typical of this position is the remark that "a gastric neurosis is no longer considered a neurosis 'of the stomach' but a more or less localized reaction to difficulties in the individual as a whole."

In the second section a comprehensive procedure of examination and diagnosis is outlined. Separate chapters are devoted to such etiological factors as the complaint, age, somatic condition, intelligence, emotional adjustment, sex life, heredity, and environmental background of the child. The third section, which is intimately related to the second, sets forth the principles and aims of psychiatric treatment, describing remedial practice with the child, family, community, follow up work, and specific therapeutic aids.

The final and largest section is a compendium of the behavioral disorders of childhood, and contains a wealth of instructive, well-chosen case material. For descriptive and classificatory purposes the concept of *ergasia* (from the Greek *ergazomai*, to be active, to work), which Adolph Meyer introduced to psychology and psychiatry, is substituted for the older notion of specific disease entities. Accordingly the terms thymergasia, parergasia, merergasia, anergasia, and oligergasia or dysergasia are substituted for manic-depressive states, schizophrenia, psychoneuroses, dementia, and amentia, respectively. Aside from the new terminology, however, the descriptive material is quite similar to that found in most textbooks of psychiatry. The author makes his chief contribution in his discussion of faulty feeding habits, emotional disorders, sexual difficulties, habitual manipulations of the body, and attack disorders in children. The psychogenic interpretation of these disorders is favored, and the precipitating rôle of the submissive or over-solicitious parent is clearly portrayed.

The principal defect of this volume is its lack of statistical concepts. In the preface Adolph Meyer warns against the replacing of "the common-sense of everyday life" by "rigid statistics"; nevertheless, there is no excuse for the presentation of long tables of percentages which could be summarized adequately with a single correlation coefficient. Furthermore, IQs are frequently cited and their comparative significance discussed with no mention of errors of measurement. Aside from this statistical inadequacy, however, the volume is meritorious.

University of Oregon LESTER F. BECK

L'étude de la pensée: Méthodes et résultats. By GEORGES DWELSHAUVERS. Paris, Pierre Téqui, 1934. Pp. 230.

In a fluent, easy style which a beginning student might read with enjoyment, Dwelshauvers raises questions that challenge every thinking psychologist. Because of his familiarity with the history of philosophy, Dwelshauvers gives an historical perspective to many of the present day psychological problems. He finds, for example, the explanation for the modern reaction against introspection in the uncritical way in which certain philosophers employed it. Nevertheless, he insists, introspection is necessary since psychological realities cannot be reduced to any mechanism that is objectively observable. Objective methods yield results of value to psychology only when they are accompanied with introspection or are interpreted in the light of it. He believes that certain psychologists who have dispensed with introspection may

have contributed to the advance of physiology and biology, but have added nothing to our knowledge of mental life.

Seeking to account for the modern confusion of detailed investigations which seem unrelated to one another, Dwelshauvers attributes this disorder to the futile attempt to reduce mental life to elements, whether the elements are regarded as mental faculties or traits, or as simple sensations or sensory qualities. Experimental psychology, trapped by a false imitation of the physical sciences, sacrificed itself, as he believes, to the element and to measurement, studied an artificial abstraction, and let mental life escape. For him mental life is a dynamic reality, always active, always complex. It is synthetic, giving coherence to our reasoning, our acts, and our conduct. Perceptions and images play a relatively minor rôle. Images are fleeting and variable. Human beings differ widely in their imagery. In their reasoning they are much more nearly alike.

Dwelshauvers of course recognizes the importance of stimuli as points of departure for thought, but he warns against confusing the sensory content of the mind with its thinking. In common with many other contemporary psychologists, he recognizes the fact that most thinking is motivated by a physical and social milieu. The stimulating object does not in most cases separate itself from desire and action, but when an intelligent adult seeks to distinguish between dream and reality, he turns his attention to his own thought processes. In so doing, he has passed from the representative-motor plane of thought to the reflective. Without minimizing the importance of studies that have been made at the representative-motor levels, the author chooses to study what he considers the most significant human achievement, rational reflection, at which level mental life is dominated by meaning.

Elements do not account for the complex structures in which they may appear. In mental life, it is the superior which explains the inferior: judgment explains concept; concept, perception; and perception, sensation. Different images may accompany the same meanings, the same image may be associated with the most diverse meanings at different times; and according to the testimony of many philosophers and a few psychologists, it is possible to have meaning without imagery. He has found in his own experiments what seems to him abundant evidence of "implicit thought," consisting of a feeling of agreement or disagreement between terms presented. This implicit thought is of brief duration and is followed by reflection, necessarily explicit. Concrete imagery appears chiefly when the thought is hesitating and meager. In general his experimental results confirm those of the Wurzburg School.

While Dwelshauvers makes no claim to great originality, his book is timely and important in an age when no discipline dares claim the dignity of being "scientific" unless it was obtained by some sort of measurement, and when the simple, direct method of introspection has been so largely replaced by inferences from very indirect observations under mechanically complicated conditions. He is especially to be commended for his incisive historical summaries linking the discoveries of philosophers and psychologists of widely separated centuries.

West Springfield, Mass. MABEL F. MARTIN

Educational Psychology. By J. B. STROUD. New York, Macmillan Co., 1935. Pp. ix, 490.

This book gives an exposition of those psychological facts and principles which have particular reference to educational problems. It treats of the conventional sub-

jects: learning, retention and recall, work, fatigue, efficiency, intelligence and intelligence testing, educational measurement, heredity and environment, mental development; and in addition there are short chapters on instinct, emotion, physiological psychology, and personality.

The treatment is from the behavioristic point of view, but Stroud does not deny the tenability of other points of view. Facts are presented as observed manifestations expressed in objective, descriptive, or numerical terms. Attempts at explanations are "founded chiefly upon neural hypotheses."

Stroud has boiled down into a readable text with well selected and organized sets of references a highly comprehensive range of material. The text alone with its successive sets of questions and exercises is adequate for a brief helpful course for prospective teachers whose training curriculum is otherwise filled with method and 'content' courses.

The modernity of the contents is typified by the brevity of the discussion and the almost summary dismissal of instinct as a factor in problems of school children, and by the relatively broader treatment of emotions and personality. Intelligence testing and its younger brother, educational measurement, receive quite adequate attention and what might be termed the evaluation of a practical, constructive critic, willing to accept evidence of serviceability yet recognizing the all-too present sources of error and unreliability in the tools of both group evaluation and individual diagnosis. Qualitative considerations are not overlooked.

The importance to educators of the concept of personality as a fluid condition of integrated behavior patterns is neatly presented and discussed. The place in the educative process of motives and interests is stressed. The most fruitful approach to the problems is cited as being by the method of observation with objective tests as the tools. Although listing as first among the profitable fields of personality testing that of interest testing the author omits any example or summary of studies using modern interest tests in his résumé of studies in personality. This appears as almost the sole inconsistency, at least of major proportions, in the content and organization of a quite usable and worthwhile contribution to the series of available text-books in educational psychology.

Stanford University KARL M. COWDERY

Child Psychology. By JOHN J. B. MORGAN. Revised Ed. New York, Farrar & Rinehart, 1934. Pp. viii, 502.

This textbook has been revised to incorporate experimental studies in child psychology made since its original publication in 1931. The author does incorporate by quotation or summary a good many of these studies. But he does not refer to, or otherwise show that he has been interested in, certain studies which seem to the reviewer of great importance. For example, in discussing "the reflexes of early childhood" he gives an account of the work of Mrs. Blanton and of the Shermans, but does not refer to the work of Weiss and his students, nor even to the relatively early work of Pratt, Nelson & Sun. In general he makes no attempt critically to evaluate the studies he does present. The untrained reader may thus be led to believe that he is learning certain "facts" when he is merely reading conclusions reached by more or less unreliable methods. To illustrate, Mateer's pioneer and very fallible study of conditioned reflexes (1918) is quoted as if it were authoritative.

Morgan follows the fashion illustrated by most popular books on "child psychol-

ogy" when he gives advice on specific problems of child care and training as if it were based on sound scientific principles, when as a matter of fact it is merely generalized common sense, or his personal opinion.

Child Psychology seems practical and realistic, but the whole tone of the book, as well as many specific statements, is over-optimistic. The book is likely to leave in the mind of the immature student the impression that a knowledge of child psychology will easily enable him to deal skillfully with children under his care, and to provide conditions that will lead to happy adjustment and successful integration. Morgan not only fails to deal adequately with the tremendous social and economic obstacles in the way of happy integration, but he also underestimates very much the complexities and uncertainties of the psychological factors in "adjustment." (See, for example, p. 235.)

The text has, however, some real merits. The style is easy, natural, and pleasing. The book deals with a host of practical problems in which students and parents are interested, and gives information and advice which should prove helpful and stimulating to a great many readers. The general point of view about problems of conduct is sensible, and the treatment well balanced.

Smith College MARGARET WOOSTER CURTI

Psychopathology: A Survey of Modern Approaches. By J. ERNEST NICOLE. With a foreword by W. H. B. Stoddart. Second edition, revised and enlarged. Baltimore, William Wood & Co., 1934. Pp. xv, 283.

This second edition of Nicole's little book is thoroughly revised and enlarged. There are four entirely new chapters (dealing with ethnology, schools of psychology and applied psychopathology) while numerous additions have been made to other chapters throughout the book. New material is included to take account of researches and developments since 1930, the date of first publication.

Nicole's *Psychopathology* is not a handbook or system of psychopathology, as the name may imply, but comprises an ambitious attempt to present a survey of all of the important theories of disorder within the bounds of a very few pages. It is frankly a summary, making little or no attempt to criticize, compare or value. Regarded purely as an outline of the more important theoretical contributions to psychopathology the book has value. At the same time, the extreme compactness of Nicole's writing and the enormous amount of literature which has been telescoped into this small volume lead to frequent elipsis and foreshortening. The tremendous pace at which Nicole dashes through his field is indicated by the fact that a mere 80 pages (the printing is rather large and the pages small) are devoted to the comprehensive theories of Prince, Freud, Adler, Jung, Rivers, Watson, Kempf, Berman, and many of their disciples and followers. The later chapters, devoted to such topics as the biochemical, physiological, constitutional, ethnological, sociological, characterological and cultural approaches to the abnormal, are even more compact, frequently taking on the appearance of a procession of names and technical terms. Although practically everyone receives at least passing mention, the researches of Jaspers, Spranger and others of the German school comprise a glaring omission.

Recognizing these difficulties and limitations, Nicole's book remains a useful survey of approaches to the mental disorders, especially for the student entering upon this wide and varied field. Its excellent bibliography, limited in scope to the names mentioned in the text, should serve as a useful guide for those wishing to become

familiar with the original sources of psychopathological theory. For those desiring detailed information on the various theories, however, as well as some knowledge of the body of facts underlying them, this book is not in place.

There are three appendices, a bibliography, and an index.

Cornell University WALTER S. NEFF

Des reflexes conditionnels: Études de physiologie normale et pathologique. By G. MARINESCO and A. KREINDLER. Introduction by George Dumas. Paris, Félix Alcan, 1935. Pp. 171.

For a psychologist, undoubtedly the most interesting part of this book is the third part, "The Application of Conditioned Reflexes to certain Clinical Problems." It is in this section that the authors report that by means of the number of trials necessary to condition a subject to a vasomotor response (measured by a plethysmograph on the arm) in one instance and a withdrawal response of the hand to electric shock, the individuals falling under the headings of Kretschmer's types, can readily be distinguished from one another. For example, it took a greater number of trials to condition the withdrawal response than the vasomotor response for the pyknic type, while the reverse was true for the asthenic. Furthermore, the authors are able to classify, by this same technique, those patients suffering from certain nervous and mental disorders. Unfortunately, Marinesco and Kreindler have not presented any similar data collected from a large group of normal individuals for control purposes. The number of subjects in their experiments, representing a given group, are small, usually five or less. The authors do not state by what criterion the subjects were designated as paranoid, dementia praecox, etc. The lack of statistical treatment of any kind, makes it impossible to evaluate the data, as meager as it is.

The second part of the book, "The Organization of Conditioned Reflexes in the Child," has been written with almost total disregard of the mass of data gathered by child-psychologists. The familiar laws of Pavlov are found to hold for infants.

The first of the book, "The Relationship of Conditioned Reflexes to the Biological Evolution of the Nervous and Endocrine Systems," is a fairly complete survey of the contributions from the zoölogical sciences and physiology, which have a bearing upon behavior, especially that which is called innate or unlearned behavior. The familiar work of Bechterev, Cajal, Coghill, Pavlov, Tilney, Herrick, Child, and of many others is noted and commented upon.

From the point of view of psychology, the book has little application, except in so far as the techniques of discriminating between mental and nervous disorders prove to be valid and reliable when subjected to a more thorough experimental discipline.

University of Illinois W. J. BROGDEN

Education of the Slow-Learning Child. By CHRISTINE P. INGRAM. Introduction by Elsie H. Martins. Yonkers-on-Hudson, World Book Co., 1935. Pp. xii, 419.

One finds this straight-forward account of the needs of the slow-to-learn child and the proposals of some means for meeting these needs directly related to the aims of the White House Conference on Child Health and Protection. By far the greater proportion of this book is devoted to a detailed account of the Special Class program of Rochester, N.Y. One sometimes feels a little overburdened by the wealth of details, which include everything from suggested "work-units"—some

fully outlined—to instruction in proper lighting, ventilation, seating arrangements, health and other armamentaria of any good school teacher. The author divides the "slow-learning" into (1) the mentally retarded (2% of the school population) whose needs are best met in special classes and (2) the borderline or dull-normal (15–18%) for whom either the standards should be lowered or more time provided for completing the regular grade school curriculum. Miss Ingram discusses each group separately with regard to the probable potentialities, the educational needs and the probable place in the community and in industry for these children. While she bases much of her discussion on the I.Q. she realizes this is but a predictive index and that there are other, sometimes more important factors involved. She rightly recognizes the responsibility of the schools to provide extension work, or some sort of follow-up for pupils who leave school at 16, but the reviewer believes that she might have develped this point further since this relatively large group of school-age people (about 10% are estimated to be in institutions) form a definite problem in community and industrial life, and they constitute, at first, essentially a school problem. The psychologist not directly connected with problems of the education of the subnormal will probably find little of interest in this book. It has a real value, however, in pointing out the increasing needs for special training not only in school but for the future of these mentally retarded individuals, and thus it should be particularly useful for educators and others directly concerned with the mental defective and the dull-normal.

Cornell University S. GERALDINE LONGWELL

Statistical Procedures and Their Mathematical Bases. By CHARLES C. PETERS and WALTER R. VAN VOORHIS. State College, Pa., Pennsylvania State College, 1935. Pp. vii, 363.

This book differs from the usual text on statistical methods in three respects: it gives (1) an unusually complete derivation of the various formulas, (2) an introductory chapter devoted to calculus, and (3) it is reproduced by the photo-offset process. The authors state in the introduction ("We hope, too, that the form of publication will pioneer a type of printing that may prove a useful means of giving to the public highly technical productions." If this is one of the aims of the book it is regrettable that more care was not taken in the preparation of the original copy: the book is filled with typing errors (which in most cases have been corrected in pencil); the intensity of the reproduction varies from page to page and often on the same page; and in several cases the alignment of various terms in a formula is such as to cause difficulty in determining what is meant. The authors have, however, succeeded in making the book available to all, as its cost is less than half of that of most texts on statistics.

The style of writing is quite informal, but for the most part the material is very readable. Certain portions of the book are unnecessarily difficult to read due to awkwardness in sentence construction rather than to the difficulty of the mathematics. The unusually complete derivations of the various formulas are among the commendable features of the book. Furthermore, the chapter on calculus presents in a very simple manner practically all the mathematics needed, so that all in all the book is the most readable of the recent books on the elements of mathematical statistics that this reviewer has seen.

The topics treated, in addition to those found in most texts, are: tetrads,

tetrachoric correlation from widespread classes, bi-serial correlation from widespread classes, and the reliability of differences when matched groups are used.

In spite of the mechanical defects, the inadequate references to other texts and sets of statistical tables, and, perhaps, the undue stress on certain functions developed by the authors, this is a book that research workers interested in the analysis of data should read if they would understand the mechanics of the statistical tools with which they work.

Fordham University JACK W. DUNLAP

Wish-Hunting in the Unconscious: An Analysis of Psychoanalysis. By MILTON HARRINGTON. New York, Macmillan Co., 1934. Pp. 189.

This book gives a simple presentation and a critical analysis of the psychoanalytic theory and technique, and a final chapter on the alternative to psychoanalysis. Briefly the theory, as elaborated by the founder of the doctrine, Sigmund Freud, assumes that the main drive to action is sexual in a rather wide sense of the term; that erotic impulses, operating in the 'unconscious,' as 'wishes,' are so much suppressed by the 'censor' (adverse reaction of society) that they express themselves in symbolic dreams, slips of the tongue, seemingly meaningless accidents of dropping things, etc., because of the inhibition of these impulses referred to in general as the libido: To the outsider the censor seems stupid; the symbols, which have deceived it, continue with their meanings unchanged, so that they are well known to the modern 'dream interpreters' and to readers of the Freudian literature. Yet it does not seem to catch and suppress the deceiving impulses which escape by means of these symbols. The Freudians in making their analyses never consider them complete until some sex drive is discovered, and at that point they always stop. This practice, of course, gives selected data.

The doctrine has become stale and rather formalized, and is losing out, according to the author. Experimental psychologists and a large proportion of psychiatrists both in Europe and in America have never accepted it. It is based on data that are not only selected, but are often untrue, and usually employed only as illustrations. The psychoanalyst thus seems more interested in justifying his methods than in studying his hypotheses scientifically. Harrington holds that, in view of the weak scientific basis of psychoanalysis, the psychanalysts take undue advantage of their patient, building up their own prestige on the patient's ignorance and dire need of help so that he accepts their doctrine uncritically. He does not deny that psychoanalysts help some people in the case of functional troubles as the usual 'faith healers' do, but he contends that results are often harmful. He proposes as an alternative to psychoanalysis (1) the breeding of a potentially healthy and efficient people (the work of eugenics), (2) medical care, and (3) educational treatment, based on a mechanistic psychology (much like the experimental physiological psychology of today) which controls behavior on the basis of suitable stimuli, both inner and outer.

The book is clearly written and should be helpful to non-technical readers.

J. P.

Charakter Leiden und Heilung. By FRITZ KÜNKEL. Leipzig, S. Hirzel, 1934. Pp. viii, 235.

This is the fifth of a series of books by Künkel devoted to his approach to the science of character. The approach in general is clinical, principles being derived

from therapeutic work with individual cases. This volume, a unit in itself, has as its object the dialectic of the individual and the group. The individual is seen to set himself off from the group. At times this natural process of individuation miscarries so as to result in neuroses or breakdowns. Pathological characters result. The group likewise separates itself from the individual, sometimes spoiling him, sometimes injuring him. From this view is evolved the rationale and some of the methods of character-therapy, particularly the use of the therapeutic group (*die heilende Gruppe*) and pedagogic therapy (*Heilpädagogik*).

The exposition in this volume is divided into three parts: (1) pedagogic therapy, (2) character-pathology, (3) character-therapy. Some of the social psychological concepts employed (the *Ich*, the *Wir*, and the *Es*) and the classification of character-disorders resulting therefrom are interesting. The use made of the group in therapy is worthy of consideration by all psychotherapists. As much of the argument is typical of all 'depth' psychologies, academic psychologists will find many of the same objections to Künkel's approach that they find to psychoanalysis generally.

University of Nebraska J. McV. HUNT

Psychometrics. By JOHNSON O'CONNOR. Cambridge, Harvard Univ. Press, 1934. Pp. xxxiv, 292.

This volume is divided into three Books. Book I demonstrates, empirically, that by averaging a sufficient number of individual measurements, a reliable measure of central tendency (in O'Connor's term, "representile") can be obtained. Book II is a discussion of statistical methods used to evaluate the reliability of measurement. Book III illustrates, in detail, the "purification" of a multiple-item test by statistical examination of the individual items.

The author's familiarity with the relevant bibliography of his subject is indicated by the fact that the total number of citations of others' work is about *four* (pp. 8, 72, 101, and 200). Consistent with such intellectual ingratitude, is the failure of the author to give credit to the numerous assistants who must have helped in amassing and treating the voluminous empirical data of the book. The volume, moreover (especially Book I and Book II) contains positive errors which cannot be described as other than elementary and shocking. The book represents a degradation of editorial responsibility by its publishers.

University of California HERBERT S. CONRAD

Mind, Self, and Society. By GEORGE H. MEAD. Edited with an introduction by Charles W. Morris. Chicago, Univ. Chicago Press, 1934. Pp. xxxv, 401.

The volume was composed by the editor from student notes and selections from unpublished manuscripts left by Professor Mead, for the purpose of systematizing the position of a leading teacher of social psychology who apparently did not himself feel the urge, or the advisability of putting his thoughts into treatise form. The book expounds Mead's ideas on the nature of the mind, and the relation of the self to society. Professor Mead's approach is from the standpoint of a modified behaviorism, in which consciousness has a place, not as a substance or an emergent, but conceived as representing a set of characters that is dependent upon the relationship of environment to a sensitive organism. Similarly 'mental images,' regarded as "memory images belonging to the environment" are retained, as is 'reason,' which Mead conceives as arising when the organism takes into its own response by means of language the

attitudes of other organisms involved. Few new insights and no new tool of analysis for the study of human behavior are offered.

The editor with great piety tried to incorporate as many of the notes and unfinished manuscripts as would make a sizeable volume. In consequence some arguments are stretched to unnecessary length, and there are many tedious restatements, all of which goes to show that lecture notes make inadequate and cumbersome book material.

Columbia University THEODORE ABEL

The Single Woman and Her Emotional Problems. By L. HUTTON. Baltimore, William Wood & Co., 1935. Pp. xi, 151.

The number of women in business has greatly increased in England since the war. The number of unmarried women has also grown, with the relative reduction of adult males. At the same time, social approvals and disapprovals touching the life of women have suffered considerable modification during the post-war years. Taken all together these circumstances aggravate the problems of the unmarried woman. The writer of this little book, Dr. Laura Hutton, discusses these problems with understanding and sagacity. A large section of the book is devoted to paired friendships among unmarried women, both the highly polarized sort where a younger woman is emotionally dependent upon an older and the less polarized kind where two women of about the same age and circumstance form a more or less lasting friendship. The dangers lurking in these friendships are set forth in a sensible way. The author emphasizes the very great difficulty of terminating the highly polarized friendship where the younger woman bestows her love upon the older to the exclusion of other social congregates and attachments. Beside these general problems, the author also touches upon the more purely sexual relations of the single woman, including means of self-relief, relief through a female mate, and by attachments to men. The book contains much less psychoanalysis than most current books upon this subject. It is astonishing to note how far description and understanding may go without appeal to Freud and the Unconscious. Of course, where the author himself suffers from 'fantasies' which lead him toward romantic and speculative explanations, the tenets and beliefs of the psychoanalyst can scarcely be avoided.

M. B.

Motorik. Edited by FELIX KRUEGER and OTTO KLEMM. Munich, C. H. Beck, 1933. Pp. 62.

This is the first issue of a new periodical, apparently a subdivision of the *Neue Psychologische Studien.* A first article, by Erich Voigt, is entitled "Über den Aufbau von Bewegungsgestalten." The title would have been clearer, and the article would have been just as clear, if the term "gestalt" had been left out. (1) People vary greatly in acquiring, a kinesthetic conception of a crooked path which they have walked strictly following a walking guide. Optical orientation during the walk is more likely to be disturbing than helpful. (2) In jumping for distance or for height in the dark, skilled athletes are at a (relative) disadvantage in comparison with unskilled persons to whom darkness is fun rather than fault. (3) Only when the subject is dissatisfied with the perfection of a complex motion, does he report "awareness" with particular details to be mentioned. When he is satisfied, there is nothing to report.

A second article, by Walter Haferkorn, is entitled "Über die zeitliche Eingliederung von Willkürbewegungen." A clockhand moved rapidly over a dial, being under cover during one-sixth to five-sixths of the circle. The subject, with an electric key, tried to stop the hand exactly under the center of the cover. Result: the subject is generally much more exact than he thinks he is. Further, the subject was asked to make a movement simultaneously with two body parts. It was found, that the left hand actually moved before the right hand, a foot before a hand, and the clumsy forehead before a foot. The subject obviously overcompensates for known lack of skill. The subject who feels "I can" loses interest in the component phases of a skilful act.

University of Missouri MAX F. MEYER

The Heroic Age of Science. By WILLIAM ARTHUR HEIDEL. Baltimore, Williams & Wilkins Co., 1933. Pp. vii, 203.

According to the author the earliest Greek thinkers lived on the plane of science, but gradually the insistent demand for interpretation led their successors to assumptions distinctly metaphysical. Thus Socrates is regarded as the first Greek philosopher. After tracing the original connection of Greek science with magic and religion and the efforts of the Greeks to free themselves from ancient beliefs, Heidel presents a rather detailed account of the methods of these thinkers, showing that the theory of induction was well developed in the medical schools of the Second and First Centuries B.C.

The volume is amply documented, and should prove valuable to those especially interested in the history of science and of philosophy. It is perhaps well, however, that the author has little to say concerning ancient as compared with modern psychology, since his statement that in regard to psychology the data of the ancient Greeks were ample and their generalizations such that modern thought has not got much beyond them (pp. 66-67) suggests an out-moded conception of this science.

Vanderbilt University EUGENE G. BUGG

Elementary Psychology of the Abnormal. By W. B. PILLSBURY. New York, McGraw-Hill Book Co., 1933. Pp. x, 375.

This elementary survey of abnormal mental phenomena conforms in the main to the conventional pattern of text-books on this subject. The introductory chapter considers the concept of abnormality, the principal general classes of abnormalities and the relations of abnormal psychology to several professional fields in which the management of human conduct is a primary problem. The subsequent topics considered appear in the following order: hypnosis, the psychoneuroses (six chapters), sleep, sex, speech disturbances, the psychoses (four chapters), the causes of insanity, feeble-mindedness, personality types, genius and insanity, and mental hygiene.

The author proposed, so he says, to write a book which would give the laymen and the college student an account of those mental abnormalities likely to be interesting to them, and which would, furthermore, show how abnormal phenomena are related to normal acts. With respect to the first aim, the book can perhaps be accounted successful since it is a clearly written descriptive survey of the facts and theories usually included in such a volume. There is, however, little real connection between the materials and concepts of scientific psychology and Pillsbury's version of 'the abnormal.' The meaning of this assertion will perhaps be clear from the

following quotation from the chapter on hypnotism: "The explanations offered for mental abnormalities have shown a gradual development from astrology on the one hand and influence of spirits on the other. One may assert that all important modern movements in the explanation of abnormal mental acts have developed from the theories of hypnotism." One wonders just where psychology comes in, and the wonder will grow with progress through the subsequent pages of the book.

Vanderbilt University LYLE H. LANIER

Adjustment and Mastery. By ROBERT S. WOODWORTH. Baltimore, Williams & Wilkins Co., 1933. Pp. v, 137.

This short book is the psychology volume of the Century of Progress Series. It is divided into thirteen short chapters. Beginning with an emphasis on the need for "human engineering to balance our physical engineering" (p. 4), the author passes to the second chapter in which he classifies the psychologically important activities of the individual under two main categories: those of 'mastery'—by which the individual gains control over his environment; and those of 'adjustment'—by which the individual changes himself to fit his environment. The psychology of mastery discusses, in two chapters, some of the conventional topics and principles of learning. Three chapters dealing with the topics of sensory activities, conditioning, set, problem solving, and emotional reactions present the psychology of adjustment. Then follow short chapters on "Differences Among Men"; "Desires and Motives"; two chapters on "Maladjustments" (the neuroses and psychoses); "Misconduct" (crime and the criminal); and a last chapter on "Teamwork" (social relations and activities).

The book is written for the layman in Woodworth's easy, fluent style. To the reviewer it seems to represent more of an attempt to picture in an elementary manner the essential features of present day psychology than to paint a contrast between the old and new methods in psychological technique and application.

University of Arkansas R. H. WATERS

Folk-Lore from Adams County Illinois. By HARRY M. HYATT. New York, Alma Egan Hyatt Foundation, 1935. Pp. xvi, 723.

This book is a compilation of the folklore of Adams County, Illinois (population ca. 63,000). The principles observed in the field work, which was carried out by the author and two assistants, all native to the county, were "knowing the people intimately and interviewing them personally." "At least 95% of the present collection was gathered within the ten square miles comprising Quincy [the county seat, population ca. 39,000] and its immediate environs."

Adams County has a mixed population. It was settled between 1820-1840 by immigrants from other States, whose ancestry, in order of numbers, was British (English, Scotch, Scotch-Irish), German, Dutch, French Huguenot, Irish, and Scandinavian. Foreign immigration, which reached its peak before 1870, increased the present German and Irish proportions to first and third rank respectively. Negroes, numbering now about 2000, entered the county soon after the Civil War. Other races, numbering altogether only a few hundred, are not included in this study. Except in the case of hoodoo and witchcraft sayings, no attempt is made to trace origins.

Nearly 11,000 sayings and instances of superstition, belief, and custom are en-

tered in the book under 160 headings. Beginning with the weather, the classificatory headings deal with plants and then animals, and then with the various phases in human life which are followed chronologically from conception, gestation, birth, infancy, etc., to death and the life beyond. A detailed index of 48 double-column pages closes the volume.

The book is a storehouse of information for the folk-psychologist.

K. M. D.

The Medical Man and the Witch During the Renaissance. By GREGORY ZILBOORG. Baltimore, Johns Hopkins Press, 1935. Pp. x, 215.

This book contains a series of three lectures delivered before the Institute of the History of Medicine at Johns Hopkins University. In them, Dr. Zilboorg discusses psychiatric thought in the Sixteenth Century. The current ideas placed mental disorders outside the field of medicine. As they were matters of witchcraft and demoniacal possession, they fell under the authority of the church and the law. These attitudes are vividly portrayed in the first lecture by means of a careful exposition of the book, *Malleus Maleficarum,* the work of two inquisitors, Kraemer and Sprenger. The second lecture shows how widespread was the belief in witchcraft, extending as it did to the medical men of the time, who considered mental disorders outside their field. The final lecture is an exposition of the work of Johann Weyer, whom the author considers to be the founder of modern psychiatry. This physician realized the pathological nature of neurotic and psychotic phenomena, and attempted to fight the current demonology. His efforts brought little immediate results, as the beliefs of the time grew out of its needs, but bore fruit in later centuries when the world was ready for them. Dr. Zilboorg's brief discussion of the psychological causes for the resistance to the newer ideas put forth by Weyer forms a fitting conclusion to an interestingly written and well documented account of an important period in the history of psychiatry. It is a book that will appeal to all who are interested in the growth of our ideas in psychiatry and abnormal psychology.

Milwaukee County Mental Hygiene Clinic GILBERT J. RICH

Religion and the Sciences of Life: With Other Essays on Allied Topics. By WILLIAM MCDOUGALL. London, Methuen; Durham, Duke Univ. Press, 1934. Pp. xiii, 263.

A more descriptive title would have been the extra-curricular interests of a psychologist. Only sixteen pages are devoted to the subject of religion. The rest are concerned with ethics, economics, eugenics, Japan, heredity of acquired characters, the lack of appreciation of psychology in England, anthropology, psychic research, philosophy, and the future of America—all considered, of course, from the special point of view of the author, but with curiously little use of psychological literature. Most of these papers have been published previously and several of them have since been expanded into books; two are condensations of books already published; and three are new. The dates scatter down through the years since 1906. There is no index.

In the paper which gives title to the volume the author asserts that there are two great affirmations of religion: that spiritual ideals have causal efficacy and that man's spiritual nature participates in a realm of spirit. Both he believes have been challenged by science and both he believes are thoroughly defensible. For the first his

defense is, however, more dogmatic than psychological and for the second he relies upon allusion to research upon extra-sensory perception (presumably that of Rhine). Little use is made of the literature of either the psychology or the philosophy of religion.

The actual contribution to knowledge is slight, but as a record of the development of the author's thinking the volume will doubtless be useful to students of psychologists and of the backgrounds of psychological systems.

Indiana University EDMUND S. CONKLIN

Readings in Psychology. Edited by CHARLES E. SKINNER with the assistance of twenty-five collaborators. New York, Farrar & Rinehart, 1935. Pp. x, 853.

As indicated in the title, the book consists of selected readings in psychology arranged under twenty-six chapter headings. Each of twenty-four of the collaborators from twenty-three colleges and universities prepared a chapter and one collaborator from still another school prepared the two chapters on learning. With but one or two exceptions, each chapter closes with a selected reference list and a series of exercises designed to test the student's knowledge of the chapter in question. Twenty-five collaborators assure a wide range of readings on specialized topics and a broad eclectic view when the selections are viewed in the aggregate.

The editor states that "the purpose of the contributors is to make available for the beginning student material that supplements the standard texts, material that is ordinarily inaccessible." In the opinion of the reviewer, *Readings in Psychology* will be an addition to libraries for ready reference and could also be used for topical discussion in a course in the History of Psychology or in a course in Systematic Psychology or, perhaps, in the latter part of a course in General Psychology where a topical method of class discussion is employed.

University of Pittsburgh W. T. ROOT

Growing Superior Children. By I. NEWTON KUGELMASS. New York, D. Appleton-Century Co., 1935. Pp. 535.

Dr. Kugelmass has written a book which he apparently intended to meet the cry of the modern parent for prescriptions in producing and bringing up their children. There is, apparently, no topic concerning the growing individual upon which Dr. Kugelmass does not consider himself qualified to speak. This confidence is evidenced by the fact that nowhere in the book does he cite his source of information. It is all the more deplorable since he makes statements as if they were unquestioned facts whereas in reality they belong to the realm of folklore. The first two pages afford ample evidence of this tendency as the following quotation reveals: "Favorable fertilization depends upon mutuality of affection expressed wholesomely. The degree of emotional devotion of one parent to the other is reflected dominantly in the transmission of the more vital elements in the constitution of the progeny. The age of the parents is of great significance. The very young tend to produce plain citizens, the very old tend to propagate genius. The greater the disparity in age, the more unusual the characteristics of the child-to-be."

This book is, in the opinion of the reviewer, of no interest to the scientist. It is not only innocuous but damaging in the hands of the credulous layman. Especially is this so since the author's name carries the added weight of a medical degree.

Normal Child Development Clinic MYRTLE B. MCGRAW
Columbia Medical Center

Gestalt Psychology: A Survey of Facts and Principles. By GEORGE W. HARTMANN. New York, Ronald Press Co., 1935. Pp. vii, 325.

This volume was written by Dr. Hartmann for the elementary student. "It is intended to be read with profit by any one who has had a first course in elementary psychology" (Preface). In its five parts are contained historical and philosophical perspectives, theoretical backgrounds, experimental studies, applications, criticisms by psychologists of other persuasions, very personal biographies, chronologies, and a glossary. Elementary students no doubt will find in this volume a complete and readable exposition of an important psychological movement.

The author has himself doubtless questioned whether the book presents a too-well rounded out system. Though he disarms criticism by stating that his task was only secondarily critical, nevertheless, the text will stimulate the alert elementary student to ask a number of questions. Is it true, as the exposition seems to indicate, that the essentially Gestalt conception is as comprehensive as the author makes out? Have all the problems and solutions claimed by the Gestaltists been originated by them? Do all the experiments cited really constitute exclusively Gestalt materials?

Other problems will occur to the more mature reader. Is organismic biology a basis for Gestaltism? Might it not be a basis rather for a non-mentalistic psychology instead of a type of mentalism such as Gestalt psychology is? Does not organismic biology clash with the Gestaltic retention of engrams as neurological explanations of psychological phenomena? As to the mentalistic problem, the reader might ask whether the claim to novelty and importance really lies in the emphasis of unity as against the additiveness of psychic factors, or rather in the possibility that all psychic factors can be extruded from psychology? Professor Hartmann's hint that it is certain that some of the Gestalt theory must be wrong only intensifies the interest in such questions, though it does not help in their discussion. Yet it is to his credit that the exposition itself prompts such questions.

Indiana University J. R. KANTOR

Roots of Crime: Psychoanalytic Studies. By FRANZ ALEXANDER and WILLIAM HEALY. New York, Alfred A. Knopf, 1935. Pp. vi, 305.

This volume contains an introductory chapter, eight chapters of detailed discussion of psychoanalytic studies and case histories of eleven male and female criminals, and two concluding chapters on the interplay of social and psychological factors and practical considerations of the cause and treatment of crime. While the book is stimulating and represents a serious attempt to put the determinants of criminal behavior on a rational basis from the psychoanalytic point of view, nevertheless, a critical evaluation of the conclusions from a strictly scientific point of view leaves many obvious discrepancies between cause and effect in criminal behavior. The main contribution lay in showing that the criminal is produced by the interplay of an individual's character with the external environment and in pointing out the probable sources of those personality traits which make a man or woman susceptible to a criminal career. The authors, in attempting to determine what traits are likely to produce delinquency, at what age they begin and how they may be modified for the better, were faced with the same dilemma that all psychoanalytic investigators face; namely, how do personality traits vary between the normal and the delinquent in reference to origin, strength and modifiability? The results of the

study are stimulating and significant in presenting a challenge to and in pointing the way for police and institutional methods of handling and preventing delinquency.

Western Reserve University ROLAND C. TRAVIS

Visual Outline of Psychology. By KATE HEVNER. New York, Longmans, Green & Co., 1935. Pp. 95.

This booklet is a very brief outline of general psychology covering the topics customarily treated in texts for first courses. The author characterizes the work as "a typical middle-of-the-road psychology" in which she drew largely from the systems of Carr and of Woodworth. The reviewer feels that the treatment has little to recommend it in as much as the particular work has been more adequately performed by recent dictionaries of psychological terms. Further, the repeated use of the term "enervation" and "ennervation" (16 ff.) where "innervation" is clearly meant, together with such doubtful statements as the nerve impulse "travels over neurone as a wave travels over a rope' (p. 9), or "The cord and the brain stem are capable of very complicated activities, but they cannot learn" (p. 13) tend to produce an unfavorable reaction to the entire volume. It seems impossible that a psychological system can be presented in such summary form without encountering the grave danger of misinterpretation.

Peabody College W. H. BOYER

Psychology and God. By L. W. GRENSTED. New York, Longmans, Green & Co., 1936. Pp. xi, 257.

Rev. Grensted's *Psychology and God*, reflects his desperate and successful struggle with the intellectual impedimenta of our time, which have grown out of the common tendency in all sciences, including psychology, to confuse a fact with its interpretation, a method of studying reality with reality itself, a small sector of experience with its totality.

The specific confusions dealt with in this book are those arising from behaviorism, the Freudian theories, the psychology of sentiments as represented by McDougall, the mechanistic conception of consciousness represented by Holt, the scientific explanation of mysticism by Leuba. Behaviorism, the author points out, while legitimate as a scientific method within the narrow limits where it applies, is not therefore justified in denying the fact of consciousness, the values of introspection, or the concepts of freedom and purpose.

A brilliant and sympathetic analysis of the doctrines of Freud is summed up by the statement: "The real danger of Freudianism lies in its mechanistic conception of the mind. The impulse of life is for him a system of forces, as closed and determinate in their application as the forces of physics." In respect to other phases of psychology, the underlying criticism is that they have envisioned mechanisms conspicuous for what they omit rather than for what they include.

The facts which no mechanistic conceptions explain include the experience of feeling, of freedom, of values, and the experience of otherness. The attempts of psychologists to explain these experiences do no more towards changing them as facts than do their methods and theories to alter the fact of consciousness. The values of character and personality, the desire of the individual to be something other or more than himself, represent facts of experience which no mechanistic conception can explain away or destroy. To these facts, only the concept of a God as a Being higher than any

particular theory of reality, can be the adequate hypothesis. God is not created in the image of man. On the contrary, he is the Ideal by which men deny the finality of all their graven images; or, in other words, God is the final appeal to objectivity, by which men save themselves from the subjective fantasies of scientific theorizing about reality.

Insofar as the hypothesis of God needs particularization, it is represented for Christians in the Life of Christ. The belief in Christ as "a crucial interpretation of reality rests upon a value judgment which psychology can neither justify nor criticize. The claim involves the interpretation of reality by human categories. These may be inadequate, but subhuman and abstract or negative categories are the least adequate of all."

The Psychological Corporation HENRY C. LINK
New York

The Foundations of Human Nature. By JOHN MORRIS DORSEY. New York, Longmans, Green & Co., 1935. Pp. xiii, 488.

The title of the book is misleading inasmuch as it is intended as a "rational" approach to the problems of mental hygiene. Its chief intent seems to be the supplying of the American teacher with "A more fundamental insight into human nature as it really is" (p. 33). That an exhaustive treatment of human nature is in no sense undertaken is shown by the citations, only a dozen or so of the 271 being to the writings of psychologists. Only casual reference is made to the field of biology, most of the quotations being directly from the fields of literature and philosophy with a sprinkling from the psychoanalysts. The psychoanalytic influence is more evident in the context, however, especially in the "analyses" of case studies (Chapter I). The reviewer is favorably impressed with the work as a treatise for the beginning teacher, unless perchance she may accept it as a summarization of the scientific findings about human nature in general, or human psychology in particular.

Louisville, Kentucky MILTON B. JENSEN

Child Guidance Clinics. By GEORGE S. STEVENSON and GEDDEN SMITH. New York, Commonwealth Fund, 1934. Pp. vii, 186.

This book is of historical nature and traces the developments in the field of child guidance clinics during the 25-year period from 1909 to 1934. Charts which show the distribution of clinics over the United States indicate that approximately one-half of the states are still without the facilities of such organizations. Some few clinics have been closed during the depression years but at the present time there are over 200 clinics, full and part time, serving the children of America.

A description of the procedure used by the Commonwealth Fund in providing an impetus for the establishment of clinics is given and a large portion of the book is devoted to a discussion of the services which are offered by the clinics today.

Wilson Teachers College WENDELL W. CRUZE
Washington, D.C.

The Ordinary Difficulties of Everyday People. By J. R. OLIVER. New York, Knopf, 1935. Pp. xii, 296.

Dr. Oliver has written a running account of the entire life course, indicating difficult and problematical turns in it from infancy to decline. Out of his very wide

personal experience he has brought a generous store of wise and friendly counsel for "everyday people" as distinguished from the seriously disordered. Many anxious youths and adults will find in the volume a good deal to comfort and reassure them. There is in the book no psychological contribution, unless the meaning of the term psychology be extended—as it actually is today in certain quarters—to the arts of divining and of human healing.

M. B.

The Return to Religion. By HENRY C. LINK. New York, Macmillan Co., 1936. Pp. 181.

In ten short chapters of this challenging book, the author ranges from playing tennis to happiness in marriage, from dancing and bridge to social planning, from the causes of personal incompetence to social security and the abundant life, from the creative employer to the destructive liberal; and in all topics he gives an interpretation that runs counter to many currently accepted educational, social, and political dogmas. The book is incidentally biographical, but chiefly an interpretation and psychological philosophy of life. Its fundamental concept is that happiness is achieved by an individual's losing himself in larger wholes, and through effort acquiring habits which will enable him to contribute something to that larger-than-self totality with which he identifies himself.

Psychologists will find in the book a novel interpretation and glorification of extraversion, a vindication of various kinds of tests when properly used and interpreted, reasonable solutions of many problem cases, and considerable evidence that the counsellor can find in religion a 'way out' for many of his most difficult cases. Case histories, reasonably interpreted, scattered throughout the pages, serve to reënforce a point of view consistently maintained and clearly presented. It is a book every clinician and counsellor should read.

Mount Holyoke College HERBERT MOORE

Crime and Sexual Development: Movement and Fixation of the Libido in Criminotic Individuals. By ARTHUR N. FOXE. Glens Falls, N.Y., Monograph Editions, 1936. Pp. 91.

This little series of case studies by the psychiatrist of Great Meadow Prison has been prepared from the Freudian point of view. Selecting 93 out of 1500 cases, the author proceeds to satisfy himself quite comfortably—authough he does say at the very close "at times I feel as if I have ventured too much on the side of speculation" (p. 89)—that, by means of one hour of psychoanalysis on the average to a case, the various crimes may be interpreted as representing oral, anal, urethral and phallic fixation. For instance, larceny of car, unlawful entry, vagrancy, are interpreted in terms of the early oral stage; late oral may be expressed as follows: patricide, matricide, fratricide, assault and murder, burglary, forgery, embezzling and disorderly conduct. Late anal: robbery with gun and swindling. Urethral: arson. Phallic: incest, bigamy. Foxe's worship of Freud is nowhere better illustrated than in the section on robbery: "Joined with the anal component there is the coprophilic utterance of a gun" (p. 33). No one who deals first-hand with criminals can for a moment doubt that sexual maladjustment and disease loom large in the production of anti-social behavior. That the vast majority of criminal actions and reactions may be reduced to oral and anal fixations is, however, a proposition that we are unprepared at this time

to accept, certainly not upon the evidence produced by Foxe, who seems not to give the least consideration to the fear-causation, for instance, which may be unrelated to the sexual factor, in any real sense, in case after case.

Clinton, N.Y. PAUL C. SQUIRES

Race, Sex and Environment: A Study of Mineral Deficiency in Human Evolution. By J. R. DE LA H. MARETT. London, Hutchinson's Scientific and Technical Publications, 1936. Pp. 342.

This highly stimulating, and one may as well add, speculative, book calls out the gamut of reactions: from irritated exasperation at the sheer boldness of sweeping generalizations with the veriest fragments of supporting data; through amused puzzlement at the (in spots) tenuous character of the argument; to thrilled admiration at the brilliant insight and ingenious attempts at new theoretical formulations. The reviewer unhesitatingly recommends it to the careful attention of anthropologists, sociologists, and psychologists. Due warning is given, however, that the author is a thoroughgoing biophysical determinist.

The burden of the book is essentially the elaboration of a theory of human biological and social evolution. The theory in brief is that the sex-linked characteristics, racial characteristics, elementary social forms and psychological characteristics are results of variance in the supply of crucial minerals in the soils and hence in the diets of animals and humans in various time periods and geographical areas. Social scientists and psychologists who are oriented to a social interactional approach will be duly irritated. Nevertheless the book deserves their critical reading. Those concerned with such problems as the ecology of racial characteristics, sex-determination, sex-linked characteristics, and the glandular basis of behavior will find the work a valuable contribution and extremely suggestive of new lines of research.

Cornell University LEONARD S. COTTRELL, JR.

Zur Psychologie des produktiven Denkens. By KARL DUNCKER. Berlin, Julius Springer, 1935. Pp. vii, 135.

The monograph examines the processes that eventuate in the solution of a problem. The enquiry is based in part on the verbal reports given by subjects as they attempt to solve a difficulty; the method is contrasted with the introspective procedure. The reports show that the solution consists of progressive reformulations of the problem and the employment of general heuristic methods. The fact that the problems are drawn largely from the mathematical and technical fields lends significance to the study.

Part II on "Insight, learning, and simple discovery" is particularly valuable for its detailed examination of insight. The concept is related to fundamental processes such as inference and deduction in mathematics and logic, and finally given a psychological interpretation on the basis of experimental findings. The discussion is a valuable addition to the current presentation of one of the central topics of Gestalt psychology.

The writer acknowledges that his conclusions transcend his experimental findings, and that the investigation as a whole is only an aspect of the larger response pattern of the individual.

Florida State College for Women P. F. FINNER

Woman's Mysteries: Ancient and Modern. By M. ESTHER HARDING. New York, Longmans, Green & Co., 1935. Pp. xi, 325.

The author avows herself as a follower of Jung and says that his teaching permeates the book "from beginning to end, for it is to his genius that we owe the ability to understand the constructive meaning of the products of the unconscious." She believes also that "ideas not unlike the primitive man's myth show themselves underlying the feelings and moods of modern man." The first two-thirds of the book is devoted to a discussion of the place of the Moon in myth and ancient religion; the last third to the Moon as a symbol. From a consideration of religious rites and myths concerned with the moon goddess, the author interprets certain characteristics of modern woman, in particular her variability, her incomprehensibility, her occasional ruthlessness and her reliance upon intuition.

The presentation of the material on myths and ancient religions is of considerable interest. Criticism of the application of this knowledge to a clearer understanding of the present emotional problems of human beings is precluded by the author's statement that "the sensible attitude would seem to be neither to credit nor to discredit the wisdom of the ancients but instead to examine these myths with an open mind. If they do not appeal to us directly they will convey no truth to us and we may as well waste no further time upon them. But if they do speak to us, their strange non-rational logic will carry conviction and no rational proof of their truth will be necessary."

University of Minnesota JOSEPHINE C. FOSTER

Psychology of Adolescence. By LUELLA COLE. New York, Farrar & Rinehart, 1936. Pp. xvi, 503.

In general, this book will make an excellent text for a course in adolescent psychology. It will also be a distinct addition as an outside reading assignment for students in child psychology who wish to do some special reading in the often neglected field of adolescence. In particular, one may criticize the easy explanations which do not explain, and the style which is chatty and sometimes downright garrulous.

Following every chapter is a bibliography of the works cited in the chapter, and one is impressed by the length of the list and its import to the subject. An appendix contains additional references not only of a scientific nature but also fiction and biography as well.

The case histories cited throughout the book—and there is a wealth of them—are excellent. They do not, as a rule, read like artificial records that are set up to be analyzed by the teacher. In the main, they carry an air of freshness and reality about them.

The author deserves congratulations for her large measure of success.

University of New Mexico GEORGE M. PETERSON

An Introduction to Experimental Psychology. By P. F. FINNER. New York, Prentice-Hall, 1935. Pp. xvi, 268.

Following a satisfactory introduction on scientific method, subject matter of psychology, and methods of investigation, 54 experiments are outlined. Although a wide variety of subjects is covered, a large proportion of the experiments will be found unsatisfactory because of their brevity and general lack of important psycho-

logical implications. Furthermore, the description of experimental procedure is frequently inadequate for both students and the inexperienced instructor. In general there is too great an emphasis upon statistics in the experiments.

The separate section on statistical procedure is for the most part satisfactory for laboratory courses. Failure to bring the tails of the frequency polygon to the base-line, however, is contrary to usage. Also the statement that "the average is . . . 221.5 times the PE, and hence is reliable," has no meaning or justification. Stimulus materials and record blanks conclude the book.

Most instructors, seeking a manual around which to center their laboratory course, will be disappointed in this book. The inadequacy lies mainly in selection of experiments, too abbreviated description of procedures, and undue emphasis upon statistics.

University of Minnesota MILES A. TINKER

Prejudice and Impartiality. By G. C. FIELD. New York, Robert M. McBride & Co., 1934. Pp. viii, 116.

The author, professor of philosophy in the University of Bristol, has written a delightful essay on the nature of and cure for autistic thinking. One will find in it no reference to the experimental literature but only the insights of an optimistic man. Field regards prejudice as the influence of any feeling on thinking except the desire to know the truth. It operates on human behavior by directing and concentrating attention and in the acceptance and rejection of decisions, and it arises from self-interest, habit, and emotional tendencies directed towards other people. The prevalence of prejudice is not, according to the author, a cause for pessimism, for prejudice is a transitional stage in thinking. It may be eradicated by the recognition that even the wisest of men may have prejudices, by subjecting opposed viewpoints to unprejudiced scrutiny, by making impartiality fashionable, and by that ubiquitous nostrum, education.

University of Oregon CALVIN S. HALL

Archetypal Patterns in Poetry. By MAUD BODKIN. London, Oxford University Press, 1934. Pp. xiv, 340.

The purpose of this book is "to bring psychological analysis and reflection to bear upon the imaginative experience communicated by great poetry, and to examine those forms or patterns in which the universal forces of our nature there find objectification."

Such archetypal patterns as the Oedipus complex determining our imaginative experience of Hamlet; the rebirth archetype in the Ancient Mariner; the archetype of Heaven and Hell in Milton, Coleridge, Virgil, and Dante; the image of Woman in Euripides, Dante, Virgil, and Milton; the images of the Devil, the Hero, and of God, in Othello, Faust, Paradise Lost, Prometheus Unbound, and the Paradiso, constitute in general the material used for analysis in the book.

The analyses and presentation are excellent; but the explanations, where any are attempted, seem inadequate to account for some very significant facts which the analyses have brought out. Underlying it all there seems to be something which may continue to constitute a very important problem for psychology; namely, the source, nature, and function of these interesting patterns to be found so deeply embedded in the mind, not only of the kind Bodkin finds basic to great poetry, but

also those forms which may be found equally important to conceptual thought, in mathematical thinking and scientific explanation, and in the organization of our perceptual experiences.

Hobart College — FOSTER P. BOSWELL

A Short History of Psychology. By WILBUR S. HULIN. New York, Henry Holt & Co., 1934. Pp. vi, 189.

In clarity and smoothness of style few books in psychology will find rank above this. In the selection of material it is about what anyone at all familiar with the subject matter would expect. Beginning with primitive animism the reader is led rapidly down through the early Greeks and the later Greeks to the religious period, the renaissance, the beginnings of experimental psychology, and then its later developments. Other fields of research are likewise outlined including the recent developments in behaviorism, configurationism, and the rest. Almost any reader will wonder why this or that was omitted and will then doubtless conclude that for the space available remarkably good judgment was exercised in the delicate matter of choice and exclusion. Many names appear in the text, frequently several to a page, and with each there is the date of the significant publication. Footnotes with these give also the full name and title of the work. A full index adds much to the usefulness of the book, especially when one observes that with each name in the index there appear the dates of birth and death.

The author says that his design was to make a history which would be an aid to the beginning student. The present reviewer believes, however, that the book will find its largest usefulness as a ready reference for dates and titles, and as a review for older students, persons who already have a sufficient background of knowledge to perceive quickly and fully when stimulated by a presentation so highly condensed.

Indiana University — EDMUND S. CONKLIN

Methoden der Kinderpsychologie und der padagogischen Psychologie. By A. ARGELANDER. (Handbuch der biologischen Arbeitsmethoden, Vol. 6, part C/II. Edited by E. Abderhalden.) Berlin, Urban & Schwarzenberg, 1935. Pp. 151.

The present article by Argelander on the methods of child and educational psychology constitutes the fifth of a series of six papers forming the second half of a volume on applied psychology. The preceding papers deal with such topics as legal psychology (by K. Haff), principles and the methods of the psychology of art (by P. Plaut), and the psychology of testimony (by O. Lipmann).

Table of contents and index are to be made available upon the publication of the sixth and concluding paper on the experimental diagnosis of feeblemindedness (by E. Stern).

Brooklyn College — EDWARD GIRDEN

The American College and University: A Human Fellowship. By CHARLES FRANCIS THWING. New York, Macmillan Co., 1935. Pp. 244.

This is not a scientific work or a psychology in the usual sense. It is an interpretation and formulation of the facts of university life based on the experience of many administrators. It discusses the personnel, organization, and administrative methods which make for success in the coöperative functioning of a university. It

is a "wisdom" book which only such a man as President Thwing from his long experience and extensive reading could produce. All persons connected with educational institutions may gain some knowledge from its pages and a great deal of wisdom such as can be acquired by consultation with experts of long experience.

Leominster, Mass. E. A. KIRKPATRICK

Buddhist Meditation. By G. CONSTANT LOUNSBERY. New York, Alfred A. Knopf, 1936. Pp. xix, 177.

Academic psychologists, if they open this book at all, will probably soon throw it aside in disgust. Terms with which they are familiar are here used with a disregard for their accepted scientific meanings so complete as to make the text practically unintelligible. But whoever is interested in the actual details of meditative exercises as clues to the nature of the much vaunted meditative experience will find not a little of interest between these covers. Here are given detailed plans, several of them, for meditation in the Buddhist fashion. Some similarities to the meditative practices of Christian mysticism can be observed but the goal appears to be far more in the mystical experience itself than is true of orthodox Christian mysticism. There is no index but there is a bibliography of other works on Oriental mysticism and there is a glossary of the Pali terms which appear so often in the text.

Indiana University EDMUND S. CONKLIN

L'exercice de la volonte. By G. DWELSHAUVERS. Paris, Payot, 1935. Pp. 250.

Previous analyses of the will have failed to reveal its essential or primary nature. For descriptive psychology, volition has its locus in the type of behavior where drives arouse tendencies to action, and where there is awareness of a goal, of the goal's value, and of the means for attaining it. This superficial orientation is sufficient for a practical study of voluntary behavior, as embodied in this book. The author sets forth in clear and systematic fashion the influences of hereditary, chemical, physiological, cultural, and psychological factors on voluntary action. The book is rich in practical observations, which are generally not new, however. A final chapter deals with metaphysical aspects of the problem, arriving at the view that volition is the key to freedom, and is the source of the identification of man with the Divine.

Wells College C. O. WEBER

Family Behavior: A Study of Human Relations. By BESS V. CUNNINGHAM. Philadelphia, W. B. Saunders Co., 1936. Pp. 471.

The title of this book is misleading if one expects to find information concerning patterns of behavior of the family as a group. It is rather human relations of the family with neighborhood and community, and of the individual in the family. It contains little new or of importance to the psychologist, although it is overburdened with quotations and bibliographical references. It purports to be a textbook for college classes, but its objectives are so poorly defined and its content is so diffuse as to make it doubtful if it will have much appeal to most college students. A book on Family Behavior which entirely omits so central a topic as husband-wife relationships misses the heart of its problem.

Cornell University DWIGHT SANDERSON

Psychologie. By AUGUST MESSER. Fifth edition. Leipzig, Felix Meiner, 1934. Pp. xii, 383.

Messer has added to the contents of his *Psychologie* in this edition, particularly in the first few chapters, which deal with the relationship and dependence of psychology upon biology and anthropology. Immediately following this is a chapter on the nervous system which, for all the relevance it bears to the remainder of the book, might be found in most any textbook of psychology by an American author. After this there are chapters on the subject matter, the problems, and the methods of psychology—curiously reminiscent of the textbooks and articles psychologists were writing when they were struggling for recognition as "scientists."

Messer adopts the Kantian division of experience into knowing, feeling, and willing as workable and adequate. Inherent, however, in all experience is consciousness "von etwas," the intentional character found in the "Akt" of Franz Brentano. But Messer is careful to point out that such an analysis, or any kind of analysis, must not lead to a false atomistic psychology. Rather, he says, "handelt es sich hier wie stets um völlig einheitlich-ganzheitliches Erleben, das wir erst künstlich zerlegen." This represents the point of view which Messer attempts to carry through the entire book of twenty-five chapters, including one chapter which treats attention as a state of consciousness as well as a dynamic force; one chapter on drives and goals; another chapter on dreams and hypnosis; and a final chapter on the relationship of the mind to the body.

One might say that *Psychologie* is modern psychology—a bit of everything. It is half textbook and half scholarly treatise. It is more argumentative and expository than organizational of experimental facts. Nevertheless, on more than one occasion Messer fails to complete the argument from a systematic point of view, finding refuge by giving someone else's opinion. The result is that the reader finds himself starting to read on another topic still a bit "up in the air" about the last one.

This book will well repay anyone who reads it with an interest in one outcome of Act Psychology. Furthermore, it is worthwhile reading if only to observe the mature serenity which Messer displays in handling and "organizing" a vast miscellany of points of view and topics, all within one *Psychologie*—including a relatively recent but "adequate" injection of Krueger's *Ganzheit.*

The Psychological Corporation, New York ALBERT D. FREIBERG

Personality Maladjustments and Mental Hygiene. By J. E. WALLACE WALLIN. New York, McGraw-Hill Book Co., 1935. Pp. xii, 511.

This work is designed as a textbook for students of mental hygiene. Part I deals with the nature of mental hygiene, and Part II with the symptoms of maladjustment, and the methods of prevention and improvement.

The author's persistent paraphrasing of ideas in successive sentences gives the book a tone of monotony and prevents any adequate emphasis of the more significant points. His discussion of the importance of physical condition to the individual's social adjustments, and of adequate behavior to physical condition is marred by the assumption of a psychophysical interactionalism. This position, taken in spite of an asserted adherence to the double aspect of mind-body, leaves the reader confused as to the author's fundamental position.

The most important contribution of the book is its wealth of case material. Nearly two hundred illustrative cases are presented. These are reports of students on their own past and present maladjustments, the factors contributing to the development of these maladjustments, and methods that have aided in their correction. Some cases which are purely descriptive weaken this part of the work. The laxity of the author's editing of the reports detracts from their value. While much may be said in favor of keeping such material as close to the original form as possible, there can be no doubt that the gratuitous interpretations that the subjects frequently gave are apt to be misleading. This is particularly true when the author incorporates such interpretations in the caption summarizing each case. An example in point is the statement that heredity is the cause of a "pampering habit of procrastination."

University of North Carolina HARRY W. CRANE

Minimum Essentials of Statistics as Applied to Education and Psychology. By DENNIS H. COOKE. New York, Macmillan Co., 1936. Pp. xviii, 271.

This book is planned for students in education or psychology who wish to acquire only a minimum of statistical knowledge for direct use in their work. The topics are organized into ten short chapters and include frequency distributions and graphs; measures of central tendency, variability, and relative position; normal probability and other frequency curves; measures of reliability; simple, partial, and multiple correlation; reliability and validity of tests; and regression equations. Questions and exercises (with answers in appendix), as well as a set of true-false statements, are appended to each chapter, so that the book may be used as a combined workbook and text. Seven common statistical tables are also included in the appendix as aids to calculation.

What, however, is the significance of the term 'minimum' in the title? One might regard certain topics as the minimum essentials, to the exclusion of more advanced and less frequently used phases of the subject. From this point of view, the inclusion of topics like partial and multiple correlation in the present book seems out of place. The student who has only a brief and superficial course in statistics should hardly be expected to carry on work that requires the computation of such measures. A second definition of 'minimum' essentials might be a familiarity with formulae and techniques of computation rather than with their mathematical derivations. In this respect, the book under consideration seems to have adhered rigidly to its purpose, all derivations having been excluded and the topics treated in very elementary fashion; it might have been advisable from this viewpoint, however, to include more short-cuts in computation, as in the calculation of correlation coefficients. Finally, one might define minimum essentials as an understanding of fundamental statistical *concepts*, with special reference to their uses and limitations in one's own field of interest. It is unfortunate that elementary treatises too often sacrifice this for the sake of developing facility in computation. There is a danger that the student becomes lost in the routine mechanics of calculation and fails to grasp the essential implications of what he is doing. The present book would probably have been much more effective if written from a point of view entirely different from that of a more advanced text, with more space devoted to a discussion of the limitations of the measures described and an analysis of the difficulties of interpretation. The use of measures of relative variability and of percentile scores, for example, deserved a more complete critical

analysis in view of their frequent misuse by educators and psychologists. Statistics is a dangerous tool to place in the hands of the layman without adequate cautions.

Columbia University ANNE ANASTASI
Barnard College

Adult Interests. By EDWARD L. THORNDIKE and others. New York, Macmillan Co., 1935. Pp. 265, including appendices of 90 pages.

The book, intended primarily for teachers of adult classes outside the usual educational institutions, consists of the interpretation of a series of investigations, partly reported and partly referred to in the companion volume, *Psychology of Wants, Interests and Attitudes*, on changes in adult interests with age and their control, a discussion of the adult education situation, and suggestions drawn from the authors' background of psychology as well as from the data reported for the selection of the curriculum content and for the setting up of effective conditions of instruction for adults. The appendices are made up largely of samples of well known tests that will be of interest to the intended readers.

The psychologist will be concerned with both the methods of investigation of adult interests in the absence of adequate techniques of measurement, and in the conclusions that adult interests resemble abilities in their distribution, have small effect on quality of memorizing, change but moderately in content with age and hardly at all in volume or intensity, and are controlled largely through repetition and associative shifting and through slight rewards rather than through punishment, just as are mental connections of thought and action. The value of the data on individual differences in the interests of adults and on changes of interests with age, derived from interest rating scales, lies in the selection of the subjects rather than in the techniques used. The several experiments reported to meet special problems were simple in set-up, carried out in small numbers, and the results reported in a semi-popular manner appropriate to the understanding of the readers for whom the book is intended.

The authors' discussion of the distribution of opportunities for adult education to the more gifted, who will acquire most to contribute to the common good, rather than their equalization for all and compulsion for the disinterested, will be of significance to all groups interested in adult education as well as to administrators and critics of the regular system of education.

To the teacher of adults the work will be a challenge to his understanding, to his point of view and to the values he may set on his efforts. This penetrating attack upon the field of adult interests, with its challenge to the present theories and thinking of many educationalists regarding adult education, will have a marked influence upon practice and direction of thinking as well as stimulate new investigations in the field.

University of Minnesota M. J. VAN WAGENEN

My Adventure into Spiritualism. By E. L. HOWARD. New York, Macmillan Co., 1935. Pp. 181.

This little book describes the experiences of a clergyman with various mediums. It is uncritical and is written from a naïve point of view. It is perfectly valueless as an evidential document, but it serves as an exhibit of what happens when observation and report are directed by the will to believe.

Johns Hopkins University H. C. MCCOMAS